ROYAL COMMISSION ON MATTERS OF HEALTH AND SAFETY ARISING FROM THE USE OF ASBESTOS IN ONTARIO

CHAIRMAN:	J. STEFAN DUPRE, Ph.D.
COMMISSIONERS:	J. FRASER MUSTARD, M.D. ROBERT UFFEN, Ph.D., P.Eng., F.R.S.C.
COUNSEL:	JOHN I. LASKIN, LL.B.

APPEARANCES:

Ms. L. Jolley, Ontario Federation of Labour

M. P. Casgrain, Quebec Asbestos Mining Association

Mr. E. Warren, Asbestos Information Association of North America

Mr. J. McNamee, Government of Ontario

180 Dundas Street
Toronto, Ontario
Tuesday,
July 21, 1981

VOLUME XX

ROYAL COMMISSION ON MATTERS OF HEALTH AND SAFETY ARISING FROM THE USE OF ASBESTOS IN ONTARIO

VOLUME XX

INDEX OF WITNESSES:

INDEX OF EXHIBITS:

180 Dundas Street
Toronto, Ontario
Tuesday,
July 21, 1981

180 Dundas Street
Toronto, Ontario
Tuesday,
July 21, 1981
VOLUME XX

THE FURTHER PROCEEDINGS OF THIS INQUIRY
RESUMED PURSUANT TO ADJOURNMENT

APPEARANCES AS HERETOFORE NOTED

DR. DUPRE: Are there any matters before I greet the witness?

MR. LASKIN: No, Mr. Chairman, other than that we are delighted to have Dr. Becklake here.

DR. DUPRE: Indeed delighted, counsel.

Miss Jolley?

MISS JOLLEY: I'm sorry. I wondered...I didn't tell counsel that I did have a matter.

MR. LASKIN: That's fine.

MISS JOLLEY: I'm just wondering, in order to be able to facilitate tomorrow's hearings, could I have a ruling on whether Dr. Kotin's submission to the Commission is in fact before the...would be evidence before the Commission tomorrow? His submission...

MR. LASKIN: Phase one?

MISS JOLLEY: Yes.

MR. LASKIN: I can't see why not. It's part of our public record...and...

MISS JOLLEY: I think we all have copies. I just wanted to make sure that...

MR. LASKIN: Certainly.

MISS JOLLEY: Okay. Thank you very much.

DR. DUPRE: I'm glad you raised that point Miss Jolley, because if any of you want to ask him questions about what he said to us in his phase one presentation, we should make sure that there is a copy available for him so that he can ...his attention can be directed to whatever he said.

Maybe Miss Kahn will have...

MISS JOLLEY: It was written by Dr. Kotin, so I presumed that he would be able to answer the questions.

Thank you.

DR. DUPRE: Well, Dr. Becklake, you are indeed welcome not only in your de jure capacity, which is to give sworn testimony as you so kindly consented to do, before this Commission, but I think even more properly in your de facto capacity, because what we are doing here is running a summer school in what we call Epidemiology 101, and you are heartily welcomed as a most distinguished visiting professor indeed, madam.

Miss Kahn, would you swear in the witness?

<u>DR. MARGARET BECKLAKE, SWORN</u>

<u>EXAMINATION-IN-CHIEF BY MR. LASKIN</u>

MR. LASKIN: Messrs. Commissioners, Dr. Becklake is going to be good enough to give us a brief opening talk illustrating some of the major points of her research, along with the use of slides, so that I'll banish to my usual place and Mr. Patterson has been good enough to run the slide projector for us.

For the record, Dr. Becklake's compendium of articles is exhibit twenty-nine, for future reference.

<u>EXHIBIT # 29</u>: The abovementioned compendium was then produced and marked.

THE WITNESS: My background is that of a chest physician, with training in respiratory physiology. I was recruited by Dr. McDonald for a multidisciplinary team which he assembled in the Department of Epidemiology and Health at McGill University to study the health effects of asbestos exposure in the Quebec chrysotile mines and mills.

I was concerned primarily with the morbidity studies in this series of...in this research program. I'm sure Dr. McDonald has touched on them, but Mr. Laskin thought it might be useful for me to review them briefly for you, and I hope you'll forgive any repetition.

As I'm sure Dr. McDonald told you, our research program had its origins in the New York Academy of Sciences meeting, under the title, The Biological Health Effects of Asbestos...I mean, the Biological Effects of Asbestos. It was an unusual meeting because of the worldwide participation of scientists from countries where asbestos is produced and mined, as well as countries where asbestos is utilized and manufactured.

It also provided a very useful review of areas for scientific study, and two items were identified as important for further study: namely, the influence of fiber type on the recognized serious health effects of asbestos exposure - namely cancer of the lungs and pleura, and fibrosis of the lungs and pleura - as well as the possibility that there might be process effects. In other words, that there might be differences between different types of exposure under different sets of circumstances.

Our data refers then to the Quebec chrysotile miners and millers, and for the Commissioners who haven't actually been to the Eastern Townships of Quebec, I'll show you some shots of the mining areas. They are about eighty miles east of Montreal, at an altitude of some seven or eight hundred feet above Montreal, in what is virtually agricultural country.

The mines are largely of the open pit variety where

THE WITNESS: (cont'd.) the rock is blasted in chunks in different shelves, subsequently broken down and milled on site.

The reason for those slides is also to underline that our own experience is that type of exposure to that fiber, chrysotile.

The research program which Dr. McDonald directed consisted of mortality and moribidity studies. The base population were the twenty-eight thousand-odd workers registered in 1966, when the study started. Anyone who had had as little as one month service in the mines since they opened in 1894, was registered.

The mortality studies, which I know Dr. McDonald has already discussed with you, consisted of...are listed here and include, of course, mortality attributable to cancer of the lung, as well as the case-control studies in mesothelioma.

The morbidity studies with which I was mostly concerned, related to the same base population and included a prevalent study on a thousand and fifteen current workers in 1967 and 1968, in which clinical measurements including symptoms, x-ray changes and lung function measurements were made, as well as followup studies on seven hundred and twenty-two of these thousand men seven years later.

They also included two lung...two studies concentrating on lung function, in which an attempt was made to understand the mechanisms and the nature of the early changes, and they included studies in which the chest radiograph was the main method of measurement, including prevalence measurements in some thirteen thousand films. The most recent of all films drawn from the files of all workers who had had chest x-rays as part of their surveillance examinations included a study of progression in two hundred and sixty-seven men, and the effects of withdrawal from exposure on eighty-six men.

The approach of this research program was to

THE WITNESS: (cont'd.) examine dose-response relationships, and I've no doubt that Dr. McDonald has already outlined to you the method used to calculate exposure.

I have added this diagram from Professor Hatch on the issue of dose-response, because I think it was perhaps he, in North America, who introduced the idea of this approach to the study of occupational lung disease, although it had been used in Britain before that.

The interest of this slide to me is that he allows the possibility of a vertical axis, which implies that there is or there may be differences in the nature of the response either in terms of a population or in terms of an individual. Thus the dose-response curve shown on the flat of the slide implies that for the dose indicated, the response rate is of the order of fifty percent of the population.

If the population had a different susceptibility or tendency to respond, if it were less responsive the line would be displaced to the right, say twenty-five percent response rate at the same dose. If it were more sensitive, the line might be displaced to the left.

The cummulative exposure index used in the Quebec studies was based on the identification of nearly six thousand jobs which were recorded in the work histories of the work force, and for each job an average dust was estimated in million particles per cubic foot, from the available measurements, of which there were about four thousand made in million particles per cubic foot years, by Mr. LaChance and his colleagues during the period 1949 to 1966. Where these were not available, estimates were made based on interviews with long service employees and information about change in process. This information was gathered and processed by Dr. Gibbs, whom I believe is to address the Commission later this week.

The prevalent studies are summarized in the

THE WITNESS: (contd.) next slide, which shows the information classified with individuals into six exposure groups, those with less than ten million particles per cubic foot years being the lefthand column, which is perhaps quite a modest exposure, through to those with eight hundred million particles per cubic foot years, which is indeed an extremely heavy exposure which hopefully doesn't exist anymore.

MR. LASKIN: Dr. Becklake, can I ask you just before you go on, because we've got to worry about keeping a record here, whether that particular slide is reproduced anywhere in any of your articles?

THE WITNESS: Yes. That comes out of the State of the Art Review. It's one of the tables in there.

MR. LASKIN: Which would be tab number seven?

MR. WARREN: I think it's table three in that.

MR. LASKIN: Table three?

THE WITNESS: It's table eight on page 217.

MR. LASKIN: Ah, good.

MR. WARREN: Of which?

MR. LASKIN: Of tab number seven.

Just so that we keep the record straight, the first slide which you showed us, or the one previous, the diagram from Hatch, is also in this tab seven, I take it?

THE WITNESS: Yes. That, I underline, is a conceptual slide.

MR. LASKIN: Yes.

I'm sorry to interrupt you. We have to worry about going back to the transcript later.

THE WITNESS: As you see from the top line in that table, there is a gradient of risk from the lowest to the highest exposure groups, of the order of threefold, which corresponds quite closely to the subsequent analyses and updating of this data contained in Dr. McDonald's New York paper, and in

THE WITNESS: (cont'd.) his most recent paper in the British Journal of Industrial Medicine.

You will notice under Other Respiratory there is also a gradient from deaths attributable to other respiratory causes. This includes pneumoconiosis, which again, if I refer you to the New York, recent New York Academy of Sciences meeting, showed a particularly high increase in rate in relation to the highest dust dose, much less marked in the five preceeding dust doses.

As regards the prevalence of radiological abnormality, you will see this also showed a gradient across the slide of quite a marked degree, both in Thetford and in the Asbestos area, and the same was true for pleural thickening, in both areas.

The symptom of dyspnea likewise showed a gradient of considerable degree, as did the symptom constellation labelled chronic bronchitis. Likewise there was a fall in lung function both for forced vital capacity, FEV^1, and diffusing capacity, which showed a gradient across the slide.

There are three points then to be made on this slide. The first is that there is in general a clear gradient in response to exposure as expressed in this cummulative exposure index at the macroscopic or the population level. One must emphasize that this is a prevalent study and it has all the shortcoming of a prevalent study because it doesn't take into account people who have left the industry, and it has no followup implicit in it, a point made by Dr. Sackett in his early evidence to this Commission.

At the microscopic level, it makes another point. If you look at the highest exposure group you will notice that thirty-four percent of men in the Thetford area showed a response or an abnormality in relation to that exposure. The other sixty-six percent of men did not show this response. That implies

THE WITNESS: (cont'd.) some factor at the individual level which is different between these two groups of men.

The second point worth making is that there are certain between-area differences, thus differences between the Thetford and Asbestos area, both exploiting in essence the same mineral deposit, show certain differences with on the whole, for instance, pleural thickening being more marked in the Thetford area than in the Asbestos area, and with a less marked difference in the small irregular opacities.

Again, there are lots of methodological reasons for these differences, but remembering that the same techniques were used and dose was measured in the same way, these differences are worth mentioning.

The third point I would like to mention relates to the function profile. If the major function abnormality in relation to exposure had been in the nature of scarring, one would have anticipated the relationship of forced vital capacity, which is the measurement of lung size, to show a stronger relationship to dust than for instance forced expiratory volume one, or FEV^1 over FEC not shown here, but which should show the same trend, which is an expression of airway obstruction.

In fact in this material both tests showed a relationship to exposure, at least implying or consistent with the view that there may well also be bronchitic type of responses in this work force, to exposure, as well as the scarring-type responses.

DR. DUPRE: All your percentages here are cummulative, is that correct?

THE WITNESS: I'm sorry?

DR. DUPRE: All your percentages are cummulative?

THE WITNESS: No. The percentage fall in lung function is the average decline in the measurement in the eight hundred million particles per cubic foot group, compared to the

THE WITNESS: (cont'd.) ten million particles per cubic foot group, taking into account age and height differences, not smoking differences. Whereas the others, the other prevalences are prevalence percent abnormality, in a given age group...sorry, in a given exposure group, taking into account age differences.

So there are slightly different figures, as indicated by the headings.

DR. DUPRE: So each of your percentages is the percentage of the exposure group?

THE WITNESS: That's right. It describes, it's exposure group.

DR. UFFEN: Counsel, I noticed that the point about the bronchitis, the graph on the screen and the graph on the paper are not the same. Bronchitis was missing in the paper.

THE WITNESS: Yes, sorry.

MR. LASKIN: Very sharp, Dr. Uffen. That's correct.

THE WITNESS: Yes, the line on bronchitis is missing.

MR. LASKIN: Well, perhaps the easiest thing to do now that you've mentioned it is simply read into the record the figures across the chart for bronchitis, which are thirty, thirty-one, thirty-six, thirty-eight, forty-six, and forty-one for the six dust exposure categories.

Thank you, Dr. Uffen.

Is that chart taken as at 1975? Is that the followup? Is that done as at 1975?

THE WITNESS: As 1967/68. That's the first prevalent study.

MR. LASKIN: 1967/68?

THE WITNESS: Yes, yes.

The followup...I'm going to enlarge on three points that arise out of this. Firstly, our followup study, secondly, a

THE WITNESS: (cont'd.) few comments on the nature of early changes, our studies in relation to early changes, and I've already enlarged, made a few comments on the issue of one-exposure profiles.

The prospective measurements to complement this prevalent data were done on the basis of that one thousand and fifteen men as the target population, because incidence or development of change over time is likely to give a much better reflection of the true health risk. We restudied as many of this thousand and fifteen as we were able to contact, and we concentrated on the indicators of fibrosis, namely breathlessness on exercise, x-ray change and function measurement, and we distinguished between the changes that had developed over time into attacks...in other words, men who had been normal in respect of one of these features when originally studied and who developed abnormality over time, and those who progressed. In other words, those who had an abnormality which progressed and became worse in the period of followup.

The next slide illustrates what happened to these thousand and fifteen men when restudied in 1974. Ninety-one had died, and as you see these men were older, they had been employed longer, had had accummulative exposure of considerably higher than the others in the group, and in addition they had had a higher prevalence of radiological abnormality...seventeen percent...and greater function abnormality, at the time when first studied.

In other words, death had occurred in those who were sicker in all respects, and had had a greater exposure.

A hundred and five men were not reseen, but we were able, restudied the rest, some by questionnaire only, some by questionnaire and lung function - four hundred and forty-five men, and in an additional two hundred and seventy-seven men, we were able to have pairs of chest radiographs in which change

THE WITNESS: (cont'd.) over time could be assessed.

MR. LASKIN: Before you pass on, perhaps we should try to identify that slide as well. I'm sorry for the interruption, Dr. Becklake.

THE WITNESS: That is in the material I sent you two weeks ago. Is that in this binder?

MR. LASKIN: That's tab seventeen. I knew I had seen it. It is table one of tab number seventeen.

THE WITNESS: In analyzing the results, we concentrated on the inference of exposure and change, trying to take into account age and smoking, the other two important issues in affecting respiratory health status.

This slide summarizes the results on all the men in terms of attack, the top panel, and progression/regression, the bottom panel, for each of the three features separately.

You'll note the solid bars indicate definite change, attack or progression, whereas the dash lines indicate doubtful change or progression. You will notice that there was a considerable portion of men who showed new abnormality in all features, and a considerable portion in whom there was progression.

The four blocks are divided by exposure according to the figures given on the X axis.

However, when you look at the form of the relationship, there appeared to be no significant relationship to dust. In other words, dust was not apparently the determining factor in attack or progression rates. It was only for the symptom of dypsnea and only for its progression that exposure, accummulative exposure had a significant effect.

By contrast...

MR. LASKIN: Could we just go back to that for a moment?

THE WITNESS: Yes.

MR. LASKIN: Let's identify it, first of all.

MR. LASKIN: (cont'd.) It's figure one in table seventeen.

THE WITNESS: Yes.

MR. LASKIN: What's the significance, in your judgement, as to the fact that attacks and progression do not respond to dust?

THE WITNESS: I would like to finish showing you the data and then try and answer your question.

MR. LASKIN: All right.

THE WITNESS: The next slide...

MR. LASKIN: Can I just clarify one thing? When you say the dotted lines show doubtful and the heavy block shows certain, does that depend on the judgement of the readers?

THE WITNESS: Again, that's defined in the paper. We tried to look at the two things separately, since for instance a change of dypsnea, which is a subjective interpretation of the breathlessness you get on effort. Of one stip we considered doubtful, of two stips we considered definite, and I think the point I want to make is that there was a lot of definite change in this population. A lot of people had developed definite abnormality in the course of the seven years...abnormality which did not appear to be related to dust as we measured it.

In the next slide, it shows the changes on x-ray, both in the parenchyma and the pleura, shown separately, and again for attack and progression/regression separately. Again, a significant number of men developed definite radiological abnormality, and yet it was only for progression of the parenchymal change that there appeared to be a relationship to dust which is evidence on the slide and which was confirmed by the statistical analysis.

MR. LASKIN: That's figure two.

THE WITNESS: Figure two.

This slide shows the significances of the

THE WITNESS: (cont'd.) relationships of exposure, age and smoking to change, attack on the top panels and progression/regression on the bottom panels. It confirms what I have just mentioned - the lack of relationship to exposure and contrasts very markedly with the significance of the effect of age in determining the outcome both for all the features, both for attack and progression.

Smoking showed a relationship to two of the features.

MR. LASKIN: Again, just for the record, I take it that slide reproduces the top part of table three in tab seventeen, although in a slightly different arrangement?

THE WITNESS: Yes, it's in a different arrangement but the data is all in there.

The reasons why there may have...we may have failed to show a relationship to dose may be one, inappropriate or inaccurate measurements of the response, or inappropriate or inaccurate measurements of the exposure or a combination of both. Or possibly that in dealing with a chronic disease like asbestosis and fibrosis of the lung, the clinical course may take a variable route. There are, of course, many reasons...which I have listed on this line, in answer to Mr. Laskin's questions...why the relationships to exposure were not stronger.

In fact, as regards response, nonspecificity of the measurement is one, that the abnormalities read on x-ray are not necessarily specific for asbestos exposure. They can be seen, to some extent, in smoking and other stimuli and other insults to the lung, although in the context of asbestos exposure probably reflect asbestosis, and there may be the issue that I've already addressed - the question of individual difference in response.

As regards dose, there seem to be far more reasons why dose would not relate to response than reasons why it should. Indeed the surprising thing is that this rather crude measurement

THE WITNESS: (cont'd.) of cummulative exposure had proved so powerful in relating to the various responses in the Quebec studies. But to list the reasons why exposure as we measured it would not be a good...would not relate to response, there is the question that we are talking about, exposure, whereas what matters is clearly the retention of the dust in the lung. We are talking about station sampling, rather than person sampling which would presumably give a much better reflection of a person's exposure. We are talking about indices developed in particles, not fibers, and it is believed that fibers are the relevant factor to the development of fibrosis of the lungs. We are talking...we did not take into account the dimensions of either and the respirability of the particles. There is the issue of exposure profile, cummulative index looks the same way whether the exposure is long and low or whether it is short and high, and then there are the issues of curve factors.

One of the most important shortcomings of the index is surely the fact that it takes no account of the profile of exposure, which in the Quebec mines is reflected in this slide taken from the work of Dr. Gibbs, which shows the changes in dust concentration over time in the worst mill - the top graph, in the best mill - the bottom graph, and on the average.

Our first prevalent studies were done in 1967, when levels were already considerably below what they had been twenty years earlier, and our followup was done in 1974. Clearly the exposure index does not reflect this change, and perhaps the reason why the relationship was so close to age is that age in the absence of further high exposures is now the best reflector of past exposure, and the relationship to age we demonstrated was indeed a relationship to past exposure.

I might add that the problem of assessing past exposures in the light of current lower levels is not unique to this population, but everybody who is doing prevalent studies

THE WITNESS: (cont'd.) and followup studies is faced with the same issue and how to deal with it in analysis.

DR. MUSTARD: Can I ask you a question? Is the worst mill in Thetford or in Asbestos, and is the best mill in Thetford or Asbestos?

THE WITNESS: You should put that question to Dr. Gibbs. This is his slide. But I think you've got it right.

DR. UFFEN: I may have missed something, but you said that there is a relationship to age. Have you shown this, in the evidence?

THE WITNESS: Yes. The previous slide in which the followup measurements were not apparently related to dust, but showed a strong relationship to age.

DR. UFFEN: Could you go over that again? I still haven't seen the strong relationship.

MR. LASKIN: I guess we can go to table three of tab seventeen.

THE WITNESS: Table three, tab seventeen. The numbers on that table are significances of relationships, and all the relationships to age on significance for breathlessness, FEC and MMES, for attack and progression, are all significant at the one percent or less level. Whereas the relationships to dust or exposure, on the righthand column, are only significant for progression, point zero two seven. That's the only one significant at the five percent level.

MR. LASKIN: When you say age, is it people getting older? Is it a particular age?

THE WITNESS: No, it's age at time of first study. We were interested to relate our findings to information which was available at the time of first study, because that is what would be useful in predicting outcome over time. What was available and what was known about the men when first studied was their dust exposure, their age and their smoking habits - which of these three

THE WITNESS: (cont'd.) factors, or could any of these three factors predict outcome over time, because that is the useful information in terms of surveillance.

MR. LASKIN: So can you translate that into a concrete example? What...does that mean there is some difference between starting work at age twenty-five...

THE WITNESS: No.

MR. LASKIN: ...as opposed to starting at age thirty-five?

THE WITNESS: Not necessarily, although that may be true. This information doesn't address that issue. It just says that the older men in this study, that that...put it this way...that certain men developed change and certain men progressed. Certain men were attacked and certain men progressed.

To what extent was the variation between individuals in attack and progression, why one man was attacked and another not, was this related to their age at the time of the first study, their dust exposure or their smoking habits?

Age showed a significant relationship when the statistical analysis was done, dust did not and smoking showed the odd relationship.

MR. CASGRAIN: Can we assume that he was still working at the time?

THE WITNESS: Pardon?

MR. CASGRAIN: Were they still in the mills when you made that study?

THE WITNESS: The first time, yes. They were all current workers, yes.

MR. CASGRAIN: But on your...

THE WITNESS: On the followup? No. That included retired individuals.

MR. CASGRAIN: But did you make a difference between...

THE WITNESS: No, we didn't distinguish between the two.

Nor did we add any dust, I might say. We didn't update the dust indices. We were interested to know what information was available about the individuals at the time of first examination, which would predict outcome.

MR. CASGRAIN: Whether there was less dust or none at all?

THE WITNESS: After that, no....nobody got updated. It was...no, the indices were not updated for several reasons. If we had had the money and time we would like to have done it, but we didn't because on the basis of this sort of information the contribution of four or five years extra dust would have been very little on top of all the accummulated past exposure. That was one reason.

But the second reason was that the study was aimed at saying what predicts a bad outcome, and if you want to predict you have to use information at the time, available at the time the first measurement was made.

So we concentrated the analysis on that question.

MR. CASGRAIN: But you always have to assume that there was no dust from a certain time on?

THE WITNESS: Well, we only assumed it for all the seven hundred and twenty men in the analysis - no dust was added. Although some of them had further dust exposure, but the dust exposure, we believe, was small and probably unimportant in relation to past exposure.

MR. WARREN: May I ask a question also, since we are having a little bit of a free-for-all here.

MR. LASKIN: Well...

MR. WARREN: Can I take two seconds, John?

MR. LASKIN: Sure. Go ahead, Ed.

MR. WARREN: Let me see if I understand this in the simplest possible terms. What we are saying is that we

MR. WARREN: (cont'd.) examined workers in 1967, and you knew in 1967 the age of the workers, their smoking history and their cummulated fiber or particle count?

THE WITNESS: Mmm-hmm.

MR. WARREN: Then you looked at the same workers again seven years later and ascertained on each of these three variables whether there had been progression...that is, whether their condition had gotten worse?

THE WITNESS: Mmm-hmm.

MR. WARREN: And what you learned was that the most significant determinant of those workers who were attacked and had more disease seven years later was the age of the workers at the time of first examination...

THE WITNESS: Of the three that we examined, of the three factors that we examined, yes.

I wish to supplement it with two points. Firstly, the prevalent studies that I showed you on the first slide had shown a relationship of abnormality to exposure. The same cummulated exposure index, inaccurate, modest, incorrect, poor as it is, had shown a relation...rather, responses had shown a relationship to the same index. We had naturally expected that outcome would also be related to that. We were surprised to find that outcome did not appear to relate to that in the same way as the prevalent studies had indicated it.

We are offering various explanations for our failure to show that relationship.

MR. LASKIN: Can I...

THE WITNESS: He has...

MR. LASKIN: And it may be...

MR. WARREN: Go ahead, John. That's the basic... I'm still back to John's basic dilemma.

MR. LASKIN: Yes, I understand all of that, but

MR. LASKIN: (cont'd.) I think what I'm trying to focus in on, when you say age, do you mean the physical age of somebody when he or she was first exposed?

THE WITNESS: No, I mean the age when we made the measurements, because we have not, in that analysis did not take into account the age when people were first exposed.

MR. LASKIN: So that when you looked at those people when you made the measurements in 1967 and 1968, you found that age was a significant factor affecting ...

THE WITNESS: No, no.

MR. LAKIN: No?

THE WITNESS: We found the dose was a significant factor taking age into account, in 1967. Looking at the cross-sectional prevalent study of working men, taking age into account, who the prevalence of abnormality was related to cummulative exposure as we had measured it.

However, when we followed the outcome of these men, the relationship to dose, cummulative exposure, seemed to be much less strong and it seemed to be much more closely related to age as they were in 1967, and my suggestion is that maybe age is becoming a much better reflection of dose in the light of this changing exposure profile. We have not examined the elements of that.

MR. LASKIN: But my problem still is, what age grouping or what age demonstrated a more prevalent rate of attacks or progressions? When you looked at the people in 1967 and 1968, I take it you divided them up into some age categories?

THE WITNESS: No, we didn't necessarily. We divided them into dust categories. Each dust category was a mixture of ages. Age on...within each...in order to correct for the difference in age, we constituted each category as if it were the average age distribution for each...for the total population...and then expressed the prevalences that we showed

THE WITNESS: (cont'd.) on the first slide, which I indicated to you were age-corrected.

In other words, that exposure gradient on the first slide might have been due to age. You might have said that it's only the older people who will reach eight hundred million particles per cubic foot years, and it was age that accounted for abnormality.

We took age into account by reconstituting the groups to take age into account.

MR. LASKIN: You standardized.

THE WITNESS: Yes.

MR. LASKIN: Then when you came to 1974, did you then separate age back out for the purpose of seeing whether it was significant?

THE WITNESS: You are asking me the methods of analysis, and I wish I had my young colleague, Duncan Thomas, with me, but I'll try and explain it to you.

Let me go back to my slide.

MR. LASKIN: You needn't. What I'm really trying to get at in the end is, in practical terms when you say as a predictor of outcome age is an important variable, you say that...?

THE WITNESS: I said in this group of men studied in this way using this measurement of dust, age was a better predictor of outcome than this measurement of dust.

MR. LASKIN: Okay. With all of those caveats.

THE WITNESS: With all of those caveats.

MR. LASKIN: All right. Which age groups when you started showed more prevalent changes?

THE WITNESS: Obviously the older age groups. This original study of a thousand fifteen men was an age-stratified random sample with much heavier sampling in the older age group where we expected to see abnormality. The ideal study would have been to sample individuals by exposure, and have equal numbers of

THE WITNESS: (cont'd.) all ages in the different exposure groups. You can't do that.

So we did a stratified random sample by age and concentrated on the people in whom we expected to find abnormality. But a lighter sample of the younger men. And there was the previous data shown in another way would confirm that.

MR. LASKIN: Let me try one more example and then I will stop. With all the caveats that you have given, if there were two men back in 1967 and 1968 that you measured, and one was age thirty-five and one was age fifty-five, based on the results that you ultimately got, what would you predict...what would the prediction be as to what would happen between those two men seven or eight years later?

THE WITNESS: I think you are asking me to extrapolate beyond what I can, but I'm prepared to try and say that if both those men had had exactly the same dust exposure, the older man is more likely to have developed abnormality in the next seven years than the younger man.

DR. UFFEN: I wonder if I could ask Dr. Becklake whether it would be appropriate now, or perhaps later, if you could explain in a general way how you arrive at table three as the one that has significance over this business of age.

For example, in the title I find something that I don't understand. It may be a very simple thing. It's called the Significance of the Relationships of Age, Smoking and Exposure to Rates of Attack and Progression/Regression, Each Adjusted to the Other Two.

Now whenever I see the word 'adjusted for', I feel that I want to understand the process through which these adjustments were made.

THE WITNESS: I greatly sympathize because I have the same problem with my young colleague, who is the statistician. But let me try and tell you how I, as a clinician, understand what he did to the data.

THE WITNESS: (cont'd.) That slide shows the prevalence of abnormality in relation to increasing dust groups. I plotted out my results without taking into account the fact that the men with the heavier dust, with the higher dust exposure were older, and they may not have had the same smoking habits. And I was delighted, because in everyone of those graphs there was a clear evidence of increasing abnormality as dust exposure increased.

My colleagues said, you are only looking at an age effect. They then...Dr. Thomas and Dr. McDonald said, you are allowing yourself to be carried away with an age effect. And they then proceeded to adjust for age by a method which is indicated in the paper, which says that each of those groups has a different component...or the men in those groups fall in different age spectra and in different smoking spectra. By making adjustment as indicated in the paper, in a statistical fashion, for these differences, they arrived at these tables of age and smoking-corrected prevalences in relation to dust.

DR. UFFEN: This is after adjustment?

THE WITNESS: This is after adjustment. And we didn't show...the paper does not contain the equivalent for smoking and age. No, it doesn't contain the equivalent preadjusted because the relationship was extremely clear and in my naivete I was carried away by it, and it was explained to me that I had misinterpreted the findings and overread age as the important factor.

DR. UFFEN: Do you mind if I just...I won't pursue this much longer, but...

MR. LASKIN: No, that's fine.

DR. UFFEN: It would seem to me then, having listened to you, that there are at least two possible interpretations of the observations. They had to go through a statistical analysis. We have been presented today with one type of analysis.

DR. UFFEN: (cont'd.) Presumably we are going to get another one.

THE WITNESS: Absolutely.

DR. UFFEN: For novices like me, I don't know whether that is significant or not. When something goes from roughly twenty-five percent to roughly fifty percent in the one called parenchyma, that's a doubling effect.

THE WITNESS: That's the only one that is significant. That's the only one that is significant.

DR. UFFEN: But it depended upon the observations of one individual?

THE WITNESS: No.

DR. UFFEN: There is a note in here that said, "one person" for that.

THE WITNESS: I don't think I understand. No, that depended on the x-ray readings of three individuals who read pairs of x-rays in two hundred and seventy-seven men.

Oh, yes. You are quite right. I'm sorry. There were three x-ray readers and the relationship was only seen for one. I'm sorry. I didn't understand you.

We had three x-ray readers and only one, for the readings of only one was the relationship to exposure shown. So I could give you two other sets of complete sets of readings which didn't show that relationship.

DR. UFFEN: Just to tidy up, has this paper been through peer review?

THE WITNESS: Yes.

DR. UFFEN: It has?

THE WITNESS: Yes.

DR. UFFEN: Is it about to be published somewhere?

THE WITNESS: Yes.

Perhaps you have not encountered the variability of x-ray reading as a phenomenon.

MR. LASKIN: Oh, yes, we have.

DR. UFFEN: What we haven't encountered is a paper which portrays the data according to the one hypothesis, which doesn't include the other data so that you could examine it separately yourself if you wished to....unless I've missed it.

THE WITNESS: We did give the results of the other readers. If you will notice on table six, we give the results for the three readers and we make the point, this is indeed one of the reasons why in a sense we were surprised that we found so little relationship to dust. It was only this one of the three readers whose results showed a relationship to dust, for one feature, and only in progression of parenchymal abnormality.

MR. LASKIN: Could I just try one final question because I think I maybe now understand it. When you say age was the important variable, you mean aging - simply getting older, no matter what age you may happen to have been. Is that what you are suggesting?

THE WITNESS: No, I think you are overinterpreting my information.

MR. LASKIN: All right.

THE WITNESS: I must go back to saying that it is age in this study, using these changes.

MR. LASKIN: But with all those caveats, though, is that what it means?

THE WITNESS: It is the...and I tried to answer it when you simplified the question and saying if you had a man of thirty-five and a man of fifty-five with exactly the same radiological changes and exactly the same dust exposure and exactly the same smoking history. This data suggests that it would be the older man who would show progression, not the younger man, if I followed him up seven years. This data suggests that.

DR. MUSTARD: Can I interject just for a moment?

MR. LASKIN: Sure.

DR. MUSTARD: To come back to my colleague in engineering's comments, I think Dr. Becklake it's fair to say that in most human population studies it's impossible to randomize and control for all the variations which exist. Because of that, there are methods of analysis that allow one to adjust for the difference in the balances among the groups. It's a very standard type of approach, there's nothing magical about it, and therefore age adjustment is a very common statistical practice. It implies no bias and indeed the studies that do not do that are at fault, and I would just like to emphasize the fact that this has been done is extremely important.

MR. LASKIN: Sorry for the long interruption.

DR. UFFEN: It's a very significant observation.

MR. LASKIN: Yes.

THE WITNESS: We would be grateful for any assistance in interpreting it.

The next point I wish to make refers to a second study we did which again addresses the importance of exposure profile, and one assumes that the most extreme exposure profile would be removal from exposure, and another paper with Dr. McDonald and Professor Liddel addressed the issue of outcome in individuals who had been removed or who had ceased exposure for any reason whatever in the period 1960 to 1971, I think it was, and in this study, which is...

MR. LASKIN: I think it's tab eight.

This is your cohort of eighty-six...

THE WITNESS: We recalled for examination individuals who had left the industry for any reason whatever, with a certain selection according to the status of their x-ray at the time they had left the industry.

The sixty-six individuals broke down into twenty who had had only two years of exposure before they left, and sixty-six who had had on the average fourteen years before they

THE WITNESS: (cont'd.) left. We looked at radiological change. Again, a comparison of the prewithdrawal film with a film taken at the time we recalled them for examination. There are difficulties about this type of analysis because with the change of time, x-ray plant has improved and the quality of x-ray films permits one to see changes which would not be readily available on the films which were taken on these men at the time of withdrawal.

Again, in this study we had seven sets of readers and we really didn't do any better in getting agreement between them.

The points however, worth making, are the following: That in the men who had had less than two years exposure, fifty percent of them were agreed by five readers to show no change over time. In other words, there had been no progression or no development of abnormality.

None of them had developed parenchymal change and four, or twenty percent, had developed pleural abnormality.

On the other hand, the men who had had fourteen years or more of exposure, thirty-six, only, percent of them showed no change whatever, or had developed no abnormality. Nine plus three, twelve percent, had developed parenchymal abnormality and twenty percent had developed pleural abnormality.

There are several points of interest here. Firstly, pleural abnormality appeared to develop in the same percentage whether the exposure was short or long, and this has been interpreted as indicating that the factors which determine pleural abnormality are the retention and...of dust, presumably subpleurally, for long periods of time. The parenchymal change developed only in those with greater exposure, and it makes a very important point that even with withdrawal from further exposure, there is a substantial percentage of men who will develop abnormality. In other words, there was no protection against further abnormality developing in those twelve percent.

MR. LASKIN: This is, just for the record, table five of tab eight.

Could I ask you just two questions about that table?

THE WITNESS: Hmm-hmm.

MR. LASKIN: The first question is, can you say anything about whether withdrawal from exposure...or does the study tell you anything about whether withdrawal from exposure slows down the rate of progression that there might otherwise be?

THE WITNESS: No, this study does not.

MR. LASKIN: The second question is, can you... are we seeing the effect of latency or simply the effect of progression after exposure ceases? I mean, can you separate out...

THE WITNESS: What...would you care to define latency?

MR. LASKIN: Well, as I...I may be below Epidemiology 101, so I...but we have heard a lot of testimony that the effects of exposure do not show themselves until sometime later. I guess my question really is, does the fact that you have withdrawn these people from exposure, is that significant or are you going to see this progression in any event because they have been exposed ten years ago?

THE WITNESS: You mean would they have progressed more...presumably the word latent means, I presume, an appearance after a period of time, which is what this study shows. All those abnormalities are abnormalities that were, if you like, latent at an earlier period.

I would very much like to have done the study along the lines you designed. In other words, to match these cases with individuals of the same exposure who had continued, but it didn't prove practical and so we had to confine ourselves to a descriptive study of looking at outcomes in people who, for one reason or another, had left the industry.

But the design of the study you suggest, I think

THE WITNESS: (cont'd.) would be an extremely interesting one.

There are, as you know, animal studies which indeed suggest that withdrawal from exposure does not only diminish the severity of abnormality, but is associated with clearance of dust load from the lungs when removal from exposure takes place.

MR. LASKIN: I don't want to get you too far away, but perhaps in the questioning we can get into that question in terms of what it says or doesn't say about removing workers from further exposure, and so on.

THE WITNESS: I think what it does say is that it doesn't prevent, which is in a sense a very tragic point.

MR. LASKIN: It makes that point.

THE WITNESS: Yes.

MR. CASGRAIN: I missed that.

THE WITNESS: It doesn't prevent abnormality from developing....removal from exposure did not prevent abnormality from developing in the men who developed it in this twelve percent of men who developed the parenchymal change, and twenty- forty percent of men, twenty percent in each group, who developed pleural abnormality.

It developed even though they had no further exposure.

MR. CASGRAIN: Because of the dust retentions?

THE WITNESS: Well, whatever the hypothesis, but it...

We did two other sets of studies directed at determining what were the earliest abnormalities associated with dust exposure, using physiological tools. The first slide is showing you data from our prevalent studies, now in graphic form, on lung function. And if you'll recall, the dust grouping from less than ten to greater than eight hundred, and the measurements of forced vital capacity and forced expiratory volume were shown

THE WITNESS: (cont'd.) in numerical form in the first slide.

What we now concentrated on was trying to blow up the lower end of this exposure/response graph, and we selected individuals who had normal chest x-rays and who had a variation or a range of dust exposures, and examined them using pulmonary mechanics to see whether there were function abnormalities that were detectible before radiological abnormalities...function abnormalities which related to dose and if they, related to dust, rather, by implication might reflect early changes.

This slide showed that in this group of some forty-odd men, neither vital capacity...which in our studies had been shown to relate to dose...nor the diffusing capacity which other people have suggested is an early responder to exposure, in this particular group of men neither of these measurements had a relationship to dose.

However, on the next slide...

MR. LASKIN: Can we just identify that slide, for the record? I take it's figure three at tab one? On page 531.

Is that right?

THE WITNESS: That's right, yes.

MR. LASKIN: Sorry, Dr. Becklake.

THE WITNESS: Sorry.

On the other hand, the static compliance shown in the top slide did show a relationship to dose, implying that in this group of individuals there was a certain stiffness of the lung which related to dose, and by implication might reflect early abnormality.

Other physiological tests suggested that the abnormality was present not in the parenchyma of the lung, but in relation to the small airways, and that would fit...

DR. UFFEN: Excuse me, could you go back? I want to make sure...you said that established...not that one.

THE WITNESS: Sorry. It's lost. There it is.

DR. UFFEN: Did I hear you say that that establishes a relationship of dose static compliance with the dust index?

THE WITNESS: I said that in this group of forty men this measurement was the only physiological measurement that showed a relationship to dust, to estimated dust.

DR. UFFEN: Pardon me. Should I be able to see that?

THE WITNESS: Yes.

DR. UFFEN: What is it?

THE WITNESS: Well, if you look in the top block... I agree it's not very impressive...but if you look in the top block, the top panel, the individuals with the lowest exposure had the highest compliance, and the individuals with the higher exposure had the lower compliance.

DR. UFFEN: I could draw a straight line through there at point two, horizontal...

THE WITNESS: Yes. I am not offering it as strong evidence that the evidence...well, as you know, the correlation coefficient is...sorry, the regression coefficient is a very modest description of a relationship, and it merely says that the...that twenty-five percent of the variation between individuals is attributable to variation in exposure. Twenty-five percent of the variation in the individual measurements of compliance in this small series of men can be attributed to exposure, and that's more than for any of the other lung functions.

DR. UFFEN: It must not be a surprise, though, for the people not familiar with your field to see very little correlation...to find that correlation figure so unimpressive.

THE WITNESS: Absolutely. It's very modest.

DR. MUSTARD: I have to interject. When you are working with data, that's not bad.

DR. UFFEN: This is not meant as personal. I'm trying to understand, and when you make a comment, you know, I can't understand this.

To what degree of reliability should the ordinary person place on the conclusions that are drawn?

THE WITNESS: I think it's a very difficult thing to present medical evidence, with all its feebleness and frailty, and yet the necessary conclusions which have to be drawn, the whole of clinical medicine is based on hypothesis, often on very much less foundation. But clinicians are obliged to take decisions in the face of uncertainty, and I think it's important for clinicians to try and present the frailty of the evidence on which we are obliged to draw conclusions.

DR. DUPRE: Physicians are like government officials, Dr. Uffen.

MR. LASKIN: Let's just identify that slide as figure four, tab one, page 531.

THE WITNESS: However, given the frailty of this evidence we were not uninterested in the fact that that, plus further analysis of the interrelationship of the physiological tests implied that the changes, whatever these changes were that the physiology was reflecting were placed not in the parenchyma or in the airspaces, but in relation to the small airways, and this slide taken from the...it's an autopsy specimen of an individual who died of asbestosis, but shows an example, a pathological example of fibrosis around a small airway which is over to the right. I guess I might point that out.

This is a large airway, and this is a small airway breaking out into...sending its authorization into the air sacs of the lung where gas exchange takes place.

THE WITNESS: (cont'd.) What we were interested in is the thickening of the wall around the small airway. We believe that dust particles which reach alveolar spaces are engulfed by macrophages, which is the body's defence against inhaled particles...that these summons leukocytes in the body's protective cells and tend to clear, some of them into the interstitial spaces, but tend to accummulate around small airways. In fact it's of some interest that the recent American Thoracic Society grading of asbestosis grades the early changes as those that occur around the small airways.

If you like, the early change is the heavy bronchiole fibrosis, development of fibrosis, and this is the criteria for early asbestosis as suggested by the American Thoracic Society Pathological Panel.

Two bits of evidence, not really strong.

DR. UFFEN: What is the little red glass bead?

THE WITNESS: I think that's a vessel.

That's Dr. Mustard's department.

DR. MUSTARD: I'll buy that.

THE WITNESS: A blood vessel. That's the blood supply to this airway.

DR. UFFEN: The black dot is a smudge on the slide?

THE WITNESS: Yes. That's dirt, I should think, on the slide.

Any other...?

The last point I wish to add is to add some comments on the study we did to address another issue, and the original 1964 New York meeting had offered the point of view that response to exposure might be affected by the nature of the fiber to which one was exposed, and by the circumstances of exposure. In other words, whether exposure took place in primary or secondary industries, and had identified this as an item for further study.

THE WITNESS: (cont'd.) It's an hypothesis extremely difficult to examine, because obviously differences between exposure in, for instance mining and for instance manufacturing, could only be deemed to occur if it could be shown that the dose-response relationship in populations exposed under these different circumstances were different. That implies that the dose in both sets of circumstances must be measured in exactly the same way, that the response must be measured in exactly the same way, and if I recall Professor Hatch's slide, that the susceptibility of the populations must be exactly the same.

If all those three circumstances are respected, then a comparison of dose-response relationships would allow one to draw conclusions as to whether process had an effect on outcome.

Obviously, under real circumstances it's impossible to make those kinds of observations.

We did, however, undertake a study in two plants in the Montreal area, with a view to seeing whether we could gather evidence to clarify whether process had an influence on outcome.

We proposed to make the measurements of response in exactly the same way as we had made the measurements in our studies in Quebec miners, and we hoped to be able to make exposure measurements in the same way. It's these two studies that I wanted to address now.

I would like to just make a point that I had forgot to make, that there was suggestive data, or suggestive evidence, in the data from the Quebec miners study on Prevalence of Radiological Abnormality, that process did have an effect because there was more parenchymal radiological abnormality noted in the small...well perhaps not very small, plant attached to one of the mines, and more pleural abnormality noted in the individuals exposed in mining, although on the average

THE WITNESS: (cont'd.) exposure in the plant was lower, an average of forty million particles per cubic foot years as compared to the miners where it was, on the average, a hundred and thirty-four million particles per cubic foot years.

That data is part of a table in the paper by Rossiter and others, including Dr. McDonald, on Radiological Abnormality in Quebec Asbestos Miners and Millers, and I imagine would be in the...in your evidence under Dr. McDonald's name.

The two plants we studied in the Montreal region were one, a textile plant in which only chrysotile and very largely Quebec chrysotile had been used, and another was a manufacturing plant in which mixed fiber had been used which included mostly chrysotile, some amosite and in the past, some crocidolite.

Again, our objective was to measure exposure and responses in as comparable a way as we had measured in the mines and mills.

As regards responses, we used the same questionnaire given by the same interviewer as in our mining study. We used the same function tests carried out on the same machinery calibrated in the same way. The x-rays were obviously taken in different places and we had different readers.

The results are shown on the next slide.

MR. LASKIN: That is table two at tab fourteen.

THE WITNESS: Yes. It contains some additional data which I think, the radiological data is not on that table.

MR. LASKIN: That's right.

THE WITNESS: In this slide, I have listed the prevalences of the symptoms in the two plants, and in the miners and millers. In order, again, to take into account differences in age and smoking habits between the two, they have been standardized to the age and smoking structure of the mining population, which is the larger population.

THE WITNESS: (cont'd.) You will notice that the symptoms of breathlessness were comparable in the two manufacturing plants and in the mines and mills. Wheezing, by contrast, was considerably more common in the two plants, compared to the mines. Chronic bronchitis showed a comparable prevalence, perhaps slightly higher in the mines and mills.

Pulmonary function abnormality, this time expressed as percentage...abnormality being defined as a given percentage reduction in function, but using the same criteria for all three work forces, showed on the whole less abnormality in the mines and mills compared to the manufacturing plant, and the radiological changes observed were, on the whole, more prevalent in the two manufacturing plants compared to the mines and mills.

What reliance can we place on this comparison? As regards the symptoms, it is surprising to us to find such comparability of symptoms, for instance of breathlessness, over different periods of time, because the miners were studied in the late sixties and the manufacturing plants in the mid to late-seventies, a period of time when there had been considerable interest and awareness of the population, both the worker and the public, and yet these figures seem to us surprisingly comparable.

Lung function is perhaps the most objective measurement, does suggest less favourable health experience in the manufacturing plants.

I think one has to be much more guarded about drawing conclusions in connection with the radiological abnormalities. In the first place, the films which describe the miners and millers were taken on old plant, and read by a different group of readers from the more modern films which describe the men in the manufacturing industry, and these, again, were read by a different set of readers. However, what evidence we haveon common reading does suggest that the three readers we

THE WITNESS: (cont'd.) used in the manufacturing study read at least comparable, and not higher on the whole, than the readers who read the films in the mining and milling study.

So that the evidence then suggests perhaps a less favourable health experience.

What evidence do we have on exposure? We attempted to assess the exposure in the insulation and cement product plant and express it in exactly the same units as we had used for the mines and mills...in other words, in millions particles per cubic foot years. We were able to do this because there had been counts done over the previous twenty years in this plant describing the same period when the men whom we studied had worked there.

However, even though the index is in the same units, it doesn't necessarily mean that it's directly comparable because the policy of sampling and technique of sampling, and so on, may well have been different between the two.

However, the evidence suggests that on the average this work force had had a mean exposure, a cummulative exposure of some fifty million particles per cubic foot years, compared to two hundred and twenty-nine million particles per cubic foot years in the mines...giving an average dust level, if you like, of two point seven million particles per cubic foot years.

When it comes to the textile plant, it was much more difficult to assess the environmental levels in relation to mining and milling. In the first place, there was almost no particulate pollution. All the pollution in this plant was in the form of fibers.

So we weren't able to give a numerical number to the dustiness years, except in a very general way, and that average dust level in the bottom line, of four point five, corresponds to something between probably two and four fibers

THE WITNESS: (cont'd.) per c.c. as an average exposure level.

We don't have equivalent figures for the mining and milling population, although a figure of...even if you use a conversion factor of one...that is, one million particles per cubic foot is equivalent to one fiber per c.c...and that is almost certainly an underestimate of an appropriate conversion factor for this work force...even if you use that figure, it looks as though the miners were exposed to an average level of two fibers...of an average fiber per c.c. level, if you make the equivalence, of eight point nine, which is double what would appear to have been the case for the textile workers.

So that all the evidence that we have collected ... oh, in addition you will notice that the textile and the men in the insulation plant had had on the average of twelve and thirteen years exposure, compared to an average of twenty years for our prevalent sample in the miners and millers. So that all of the evidence that we have collected suggests that the environmental pollution was to lower levels for the two groups of men in the manufacturing plants, compared to the miners and millers, and yet their health experience was less favourable.

DR. DUPRE: Dr. Becklake, was there a mortality study of the employee population for those two...?

THE WITNESS: Yes, there was. Yes, with both of them.

DR. DUPRE: Does that mortality study bear out, again, your finding that the nature of the exposure is...

THE WITNESS: I have to be guarded about that. The mortality study on the textile plant was the subject of a student thesis a number of years ago where the director of the study was Dr. Gibbs, and the followup period was considered too short to be useful for drawing serious conclusions. I believe it is Dr. Gibbs' hope...it is felt that a followup when there was a

THE WITNESS: (cont'd.) bigger...which would add another seven or eight years to the followup would make that a much more useful study.

But he, as the supervisor of that thesis, would be the correct person to ask about it.

In addition, a second student of his did a mortality study in the manufacturing plant, and again I don't...I believe he may have just submitted that thesis and perhaps it would be...you might ask him about both those mortality studies.

So the conclusion out of that study was that it is certainly consistent with a fiber and/or a process effect, but it's by no means conclusive evidence, but it's consistent with that original hypothesis. But clearly the evidence is very modest.

MR. LASKIN: Do you say it's...I understand the point you made about process. Do you say your conclusions are also consistent with, or your findings are consistent with drawing some conclusion about the different effects of different fiber types?

THE WITNESS: Well, no. One of those plants was only chrysotile, and so that...the comparison with the miners and millers there would be consistent with the process effect.

The other plant included mixed fibers, so whether that is a fiber and/or a process effect, we were unable to separate out.

In the analysis, we had hoped that we would be able to identify parts of that plant or jobs in that plant where there was no exposure to one fiber. But it was an open floor sort of plant, so we couldn't exclude anybody as never having exposure to any fiber, and were unable to separate out the fiber effect then.

So that might have contributed to any differences we showed between the miners and the...

MR. LASKIN: So the conclusion is that it may have been an effect, but from your findings it's not something you can say one way or the other?

THE WITNESS: No, no. It's consistent with the fiber and/or process effect in relation to the second plant.

Finally, if I could summarize the points that I've tried to make, which are in a sense the summary of the presentation I made at the more recent New York meeting, firstly our studies have shown a relationship between cummulative exposure and serious disease and death.

Secondly, that the prevalence of abnormality does increase with cummulative exposure, but the relationships are relatively weak and appear to be only a partial explanation of our findings.

I have offered several suggestions as to why this relationship may not be stronger, and it is probably reasonable, therefore, to offer the conclusion at least that controls should be based certainly on environmental monitoring with health monitoring playing a complementary role, but clearly environmental monitoring becomes an important component of control.

Finally, I made the point there, and I think the point still stands, that the clinician is left with the dilemma of how to advise the subject who has questionable changes in a health examination, and I think that decision has to essentially remain a clinical decision which is an assessment of all the available evidence, and making the best possible advice taking all this into account.

I am in sympathy with the view expressed by Dr. Weill in his summary of the Lyon Conference, and if I quote him, it is that radiological evidence of asbestosis should lead to the prudent course of avoiding further exposure, and that this course of action may not be necessary for workers who exhibit

THE WITNESS: (cont'd.) limited benign pleural abnormality.

I would be in support of interpreting...or at least would be in support of the view that he expresses on that occasion.

I think that's all I have to say, thank you.

MR. LASKIN: Thanks, Dr. Becklake.

Just on that last point, do you subscribe to what I understood was the hypothesis behind that view of Dr. Weill, and that was that fibrosis responds to dose, increasing dose, whereas pleural changes respond primarily to time since first exposure?

THE WITNESS: Yes, that was implicit in the slide on our data on the withdrawal study, which is very close to his own study on the effects of withdrawal from exposure.

In other words, the prevalence of pleural abnormality appeared even after rather short exposure, down the line. Whereas parenchymal abnormality did not appear in individuals who had had the lower exposures, and therefore I think he developed that view on the basis of the fact that it would be...that if you can maintain the exposure lower you have a less chance of developing abnormality, and presumably of progression.

MR. LASKIN: Q. Can I ask you, bearing in mind what you said about the prudent course, as a practicing chest physician have you, yourself, developed any of your own criteria as to when you advise a particular worker exposed to asbestos to remove himself or herself from further exposure?

THE WITNESS: A. In expressing my sympathy for Dr. Weill's point of view about prudence, I forgot to mention a point that I think is very relevant these days, and that is that in many workplaces asbestos is either banned or exposure levels are very much lower than they were. Whether the worker

A. (cont'd.) follows your advice or not will surely depend on that sort of additional information, which very often the practicing physician is not in a position to provide. That information is more readily available to government agencies. In Quebec, it's more readily available to the Department du Sanite Communite, whose job it is to have information about the environmental situation in the plants in its particular region.

Other than share with Dr. Weill the dilemma on how you advise the individual, no. I can't, I don't think, say anything other than I believe that each case has to be judged on the basis of all the information about it. And as I said, all clinical decisions, which are mostly taken in the face of uncertainty, mostly taken on the basis of inadequate evidence, and mostly taken on the basis of best judgement weighting, trying to put value on the relative quality of the different pieces of information that you have about an individual.

I don't believe it's possibly to codify that kind of decision making, if that's the question you are asking.

Q. That's fair enough.

Is there, just on that point, is there, in your judgement, any point up to which fibrosis or some fibrosis is to any extent reversible? I mean...?

A. It's a very interesting question. I think as we understand fibrosis in the pathological sense it probably is not reversible.

On the other hand, there is evidence from animal work, and also from human work, particularly using bronchoalveolar lavage and the analysis of cells that can be washed out of the airways, that there are certain fibrotic, diseases which eventually become fibrotic in man, in which there is a stage of activity at which intervention in these other diseases, usually with steroids and so on, does prevent progression to fibrosis.

In other words, now looking at the whole spectrum

A. (cont'd.) of fibrotic diseases in man, due to all the other causes besides asbestos, there is developing opinion that there is a stage of activity where they are susceptible to intervention. Whether or not it will be ever possible to identify that stage in the asbestos-exposed individual, I don't know.

It seems unlikely because on the whole the fibrosis associated with asbestos exposure is very slow in developing. It doesn't have a very active phase, which most of these other diseases that I'm talking about do. Further more, particularly in the future with exposure levels being so much lower, presumably it is...becomes a much more latent process. Not latent, it's not the word, but a much less aggressive process.

But it's certainly within the area of hypothesis, but I would be interested in the opinion of the Commissioner who is a pathologist about whether fibrosis as you see it is a reversible procedure.

Q. I took the point from one of your articles, your review article, that at least when you wrote it there was no effective treatment for fibrosis itself, as opposed to...I think you made the point that you could deal with some of the symptoms.

Is that still the case in 1981?

A. Yes, I think that's the case. Yes.

If you mean by fibrosis the pathological end point. Fibrosis is scarring, it's the end point of a number of processes, but you don't reverse that.

Now, it doesn't mean to say that...in any one individual the disease that you see on x-ray might be a mixture of scarring, active inflammatory areas, partly fibrosed areas. I suppose it is conceivable, if not quite likely, that the active areas, I presume, or perhaps the areas that are newly under fire by dust, the old areas represent the end stage...there is also the interesting point to which people like Dr. Weill and Dr. Turner-Warwick subscribe very strongly in England, that whereas

A. (cont'd.) dust, the dose or exposure dose determines the initial response, perpetuation of that response in the individual depends on other factors inherent in the individual. Perhaps factors which evoke an autoimmune or some sort of change of the immune behaviour of that individual once the disease process is started, so that the continuation of the disease process has been, I think, very graphically described by Margaret Turner-Warwick as perpetuating circuits independent of the original agent responsible.

So that, then, addresses a slightly different issue.

Q. Does that suggest that once you give...what you have to do is give a person a certain dose, depending on his or her individual susceptibility, and beyond that these other factors take over?

A. That would be their hypothesis, I think, but developed mostly on the basis of other disease, but with a certain amount of experience in asbestos-exposed individuals as well.

Q. Do you, yourself, have any judgement one way or the other on that hypothesis?

A. I think that it's a reasonable hypothesis.

Q. Just to come back to the question of treatment, in its practical sense if someone in the workplace has evidence of moderate, even relatively severe asbestosis, is there effective treatment of the symptoms in a way that will allow that person to lead a relatively decent life?

A. It's all a matter of degree, because the degree to which your life is impaired depends on whether your exercise tolerance is impaired, whether you can carry on with... whether you can care for yourself, whether you can walk upstairs. What is the reserve? How much of your lungs' enormous reserve... because you have an enormous lung reserve...how much it's impinged upon.

Doctors are used for caring for individuals with

A. (cont'd.) diseases that are not reversible, and there are things you can do. You can protect them from infection. Tuberculosis is no longer a real threat in Western societies, but it is in developing countries, but it's no longer a threat here.

Control of the cigarette is obviously important in terms of risk for developing lung cancer. I don't think anybody has studied whether the lung cancer risk in individuals with asbestos-related fibrosis is diminished if they cease smoking, in the same way as it has been done for regular cigarette smokers without fibrosis. You know, if you quit smoking your risk falls within fifteen years to as if you had never smoked. Does the same happen for asbestos workers? I don't know.

There is some evidence from Sheers and other individuals that quitting smoking diminishes your chance of developing certainly pleural abnormality and possibly parenchymal abnormality, but it's not strong evidence so that if one advises a patient you certainly advise them about quitting smoking because of its known effect on lung status.

Yes, other treatment, there is no curative treatment. There is treatment in the form of care, but then a lot of medicine is. I think we were brought up at medical school to say that twenty percent of the individuals you will see, you will cure, and eighty percent you might offer some care to.

Q. I wonder if we could take advantage of the fact that you are a chest physician and perhaps I can pursue with you one or two issues which have come up during the course of the hearing which really relate to mechanisms in the lung, and so on?

One matter that has come up is this; Dr. Enterline, who was our first witness, put to us a proposition that there may be a difference in the way in which a person receives dose, in the sense that if you get a very intense dose all at once as opposed to being spread out over a longer period of time, that may be more

Q. (cont'd.) hazardous, because you may in some sense be overwhelming the lungs' defence mechanisms or clearance mechanisms. Is there any medical or biological support for that theory?

A. I think the question of exposure profiles and their influence on outcome is an extremely important and interesting one. The clinical evidence for Dr. Enterline's point surely comes from the data on exposure histories, particularly of the domestic exposures in relation to mesothelioma - the women whose exposure has been to shaking out their husbands' workclothes.

Dr. Newhouse, a colleague of mine in London, measured fiber counts in relation to shaking dust out of workclothes and found them to be extremely high. If you consider that kind of exposure, which is essentially peak exposures, then I would have thought that there is evidence here and they are very much in support of Dr. Enterline's hypothesis.

I am not aware that anybody has studied profiles of exposure in relation to the chronic...to fibrosis of the lungs, to asbestosis. We had wanted to and indeed tried to mount such a study to see whether exposure profiles, in particular peaks and troughs, might have accounted for some of the between differences in our individuals. It's underway. I doubt if it's going to show anything.

But I do think that it is quite...I think that it is likely and possible, both on the basis of scattered evidence and on the basis of what is known about lung clearance mechanisms, that large doses might indeed permit greater temporary retention.

Q. Can you just briefly elaborate on this point about lung clearance mechanisms? What is known about it as it relates to the ability of the lung to retain asbestos fibers or clear them?

A. With great modesty, particularly in face of the audience amongst whom there is somebody who is...Dr. Muir

A. (cont'd.) has studied and written a great deal about lung clearance mechanisms, and I know has given evidence to your Commission, and I'm sure he has told you about how the penetration and retention of particles relates to their size.

I'm sure he has told you about between-individual variation in clearance.

Q. We haven't actually had the benefit of having Dr. Muir up in the same chair that you are in, yet, Dr. Becklake.

Can you elaborate on that a little for us though?

A. Larger particles, probably down to seven or eight microns in length, impact on the nose and the upper airways, are trapped in mucous and are probably cleared fairly rapidly by what is called the mucous escalator.

Smaller particles penetrate more deeply and into smaller air spaces, and the very fine particles probably get out into the peripheral air spaces.

What determines whether they are deposited there or whether they come out again, because they are so light, depends on their physical properties and the aerodynameter, which determines whether or not they are going to settle. So that physical properties probably determine penetration and deposition of particles.

In the case of asbestos, there is some interesting work which I wonder if I can recall with sufficient precision, but might be able to look up over the course of the lunch hour. But the particles of chrysotile, which penetrate into the lung space, which are deposited, engulfed by macrophages as part of the protective mechanism, and then enter into the lung substance either as free particles...probably the smaller particles can enter as free particles, the larger particles tend to be engulfed by macrophages...but they distribute themselves. If they land peripherally in the lung, they may be cleared by the peripheral lung lymphatics and settle underneath the pleural surface of the

A. (cont'd.) lungs. Otherwise they tend to be cleared centrally via the lung's lymphatics, to the lymph nodes, passing through the lymphoid collections at the level of the small bronchioles, of which I showed you a picture earlier.

Now what happens to chrysotile is unusual, because as you know its physical and chemical properties presumably permit it to be destroyed or undergo degradation in the lungs, in contrast to amosite and crocidolite, which persist in the lungs... as far as is known, based on the recovery of lung dust.

The chrysotile fibers are distributed differently in the lung from amosite fibers in terms of size, and it's that point I would like to check on. I think that the smaller chrysotile fibers are found in collections underneath the pleura, whereas the fibers that are found in the lung substance are larger in size.

Amosite, on the other hand, shows no size preferential distribution between the subpleuaral area and the parenchymal area of the lung.

What does that mean? I'm not sure in terms of its significance, but what it does tell you is that the management by the lung of these different fibers is determined by a number of different factors. It's a long way...long, roundabout answer to your question, and I'm not even certain that I've answered it.

Perhaps you would like to put another one and I'll see if I can get back to the point.

Q. Well, the different factors. I take it the first different factor that you mention is fiber dimensions?

A. Yes.

Q. You also mentioned chemical properties. Now what's the difference in chemical terms between chrysotile and the amphiboles that may affect the retention factor?

A. I don't know. Penetration deposition is probably physically determined by size. Retention...not retention in the lung, but survival in the lung, appears to probably be

A. (cont'd.) related to chemical properties and is the hypothesis offered as to why chrysotile appears to be leached out of the lungs. Because as you know, whereas for crocidolite and amosite the amount of fiber recovered from the lung increases with exposure, with chrysotile it doesn't. It stays flat after a given level, implying that the stuff gets into the lung, but in some way disappears. I don't know of hypotheses, what the explanation for this is. Again, I suggest you direct your question to Dr. Gibbs later in the week.

But this is how that is interpreted, the observation of lower chrysotile...the lower amount of chrysotile recoverable from the lungs of individuals with long exposures.

DR. UFFEN: Perhaps you could explain something for me that's related to ...we've heard about ferruginous bodies. Does that mean that they have iron in them?

THE WITNESS: Yes. They have iron in the coating. A fiber, when it's inhaled, if it's long, is attacked by a lung macrophage. Asbestos fibers are so long that they probably take two or three macrophages to deal with them. In the reaction to the fiber, the macrophage appears to cover it with a protein coating which contains iron...the iron possibly coming from red cells, but it is not known where the iron comes from that makes this protein coating.

DR. UFFEN: Does it not come from the asbestos?

THE WITNESS: I don't think it is believed to come from the asbestos body. I think it is believed to be supplied, so to speak, by the body.

DR. UFFEN: The thing that puzzles me and I would like to get it clear, is in the amphiboles is iron...

THE WITNESS: Yes.

DR. UFFEN: ...and in pure chrysotile is magnesium.

THE WITNESS: Yes. You realize that the reason for the name ferruginous bodies was that the word, the 'ferr' part

THE WITNESS: (cont'd.) of the word describes the covering, the iron protein covering of these bodies, and it was realized that the content of these ferruginous bodies is by no means always an asbestos fiber. It might be a cotton fiber, it might be a talc fiber, it might be all sorts of other fibers.

However, in practice the EDXA, the energy diffusive x-ray analysis, which allows you to precisely identify the fiber inside these ferruginous bodies, reveals that most of them are indeed amphiboles, interestingly enough.

DR. UFFEN: Not chrysotile?

THE WITNESS: Not chrysotile, although chrysotile is the most commonly used and exploited fiber, studies by people like Andrew Churg, now at the University of British Columbia, and studies from elsewhere...I think Pooley's studies also...have shown that in women the common content of the ferruginous body in the lungs are talcs and a variety of other fibers. In men, the common content of the ferruginous body is the crocidolite and amosite or the amphiboles, whereas chrysotile is the material that is most used, most exploited, and to which presumably most people are exposed.

Presumably this is just another expression of the fact that chrysotile is degraded within the body.

DR. MUSTARD: Can I just ask you a question in relation to this dialogue? One would be left with the impression that if you did an attack progression study on individuals exposed to amosite fibers, that you might find a much greater attack progression story. Has anybody done such a study?

THE WITNESS: No.

DR. MUSTARD: But there would have to be an expectation...

THE WITNESS: It would be an expectation. The only issue is, however you did the study you would get the same load of criticism that our own study has got - that you have no

THE WITNESS: (cont'd.) right to make a comparison between miners and the factory workers, as we have done, because of the differences in the techniques of measurement both of dose and of response. And the difficulty would be again to design the study where you could gather information that allowed you to compare it with anything else.

If you tried to compare it, they would say well, you have no right to make those comparisons, the methodology is different, the time is different...one study was done seven years ago, one study is done now...things have changed.

I'm sure you are familiar with the criticisms which your epidemiological colleagues will offer.

DR. MUSTARD: That's why, I understand, why lawyers are involved in cross-examination because it seems to me that you can equate the two....

MR. LASKIN: We may all resign en masse...except Linda.

DR. UFFEN: Having interrupted, may I ask another question?

MR. LASKIN: Sure.

DR. UFFEN: There are things I'm finding out that bother me. There are macrophages. Are there such things as microphages?

THE WITNESS: No. There are white cells, which I'm sure you have heard of, white cells...leukocytes...white cells in the blood. These are the body's other...they are the ones that come and eat. They are the ones that the macrophages summon. As the fibers come in, the macrophages attack and fiber and they send out a chemoattractive material which summon up the white cells.

The white cells are probably the prime...they are the prime killers, they kill bacteria that come into your lung. They also release enzymes which may destroy your lung. In the case of smoking for instance, it is hypothesized that the cigarette

THE WITNESS: (cont'd.) smoke also turns on, to use the modern terminology, the macrophages, which then also summon up white cells, and their sequence of reactions there is slightly different, but the key to the lung's response is the macrophage and its summoning up of all the white cells and all the consequences that that entails, with the various enzyme releases and so on that occur. I find extremely interesting that the two thrusts in understanding lung disease come one, from the study of the macrophage and at the cellular level, and two, from the epidemiological studies which are macro in the degree. It's quite hard to put the link between the two...at what stage does the turned-on series of macrophages turn into a disease, which is where the clinician sits.

DR. UFFEN: What role do the white cells play when the macrophages summon them to a ferruginous body?

THE WITNESS: No, the ferruginous body is a later stage. You mean what role do they play in the generation of a ferruginous body? I don't think any, but I would have to look that point up.

DR. UFFEN: I may have been incorrect. I just wondered, what is the role of the white cell when the trouble is caused by a silicate fiber?

THE WITNESS: I don't know if that is known. I certainly don't know it. I could refer you to two extremely good review articles, one by Dr. Turner-Warwick, and one by Ron Chrystal, from the National Institutes of Health, which have tried to look at the starting point of these interstitial lung diseases, and address that issue. I don't think I can answer your question.

MR. LASKIN: Q. Just following this process of macrophages and coating, in your review article you raised the possibility that once this happens that the fiber will then become nonfibrogenic.

THE WITNESS: A. That hypothesis has been put forward, and I noted was repeated again in a recent review on ferruginous bodies, by Churg, and it's based on animal work.

The belief is that the ferruginous body takes weeks. I think the figure given is somewhere between twenty and forty weeks to form, and in that form may be inactive. Again, I have not reviewed all the experimental evidence, of which there is an enormous amount. The number of publications relating to animal work on this particular area is enormous, in the last four or five years.

I should think the literature must have more than doubled, and I guess you have information on this, in the last four or five years since I wrote that review. But the one point that Dr. Churg made which I found quite interesting was that it is conceivable that the asbestos fiber may produce it's coated.

In other words, it may produce an immediate...it may have some role in turning on the lung's responses, before it becomes coated. He also subscribes to the view that once it's coated, it remains inactive.

There is some evidence that coating turns over, that fibers may subsequently become uncoated. Again, I don't know how that evidence stands at the moment. It's obviously animal evidence.

Q. When you say inactive, do you mean noncarcinogenic as well as nonfibrogenic?

A. I don't know if any work has been done. I meant nonfibrogenic. I don't know if there is any work to support the view that it would be inactive from the carcinogenic point of view.

DR. DUPRE: Can I just pursue this in a specific way, Dr. Becklake? If I look at your paper in tab seven, I believe that is the one that is being referred to, and I'm looking at page 196, the lefthand column on page 196, just before the heading

DR. DUPRE: (cont'd.) Cellular Effects. There I read the following: "For instance, it has been shown that although

> all fiber types may become coated in the laboratory animal, in man it is the amphibole fiber that is found more frequently as the core of the ferruginous body than the chrysotile fiber, even when both types of uncoated fiber are seen in the lung. The significance of this observation remains to be determined. It may simply represent the relatively high solubility of chrysotile in relation to the other asbestos fibers".

Now, when you speak, Dr. Becklake, of the high solubility of chrysotile, are you associating that with the extent to which chrysotile may have much less acid resistance than other types of asbestos fiber?

THE WITNESS: I think you are attributing to me a much greater knowledge of the physics of particles than I really have. I think that's...what I meant there...the fact that chrysotile fibers seem to disappear from the lung. They also appear to diminish in size. If they are leached or if they are susceptible to degradation, they might diminish in size, and a small fiber is less likely to be coated than a large fiber.

So if you want a hypothesis, the larger fibers might get in and they might degrade, become small and therefore no longer be susceptible to becoming a ferruginous body. That is one hypothesis that has been offered.

DR. DUPRE: You see, as someone who is just taken to this point and knows nothing about either clinical medicine or physics, what prompted my question was very simply that a physicist, Dr. Chatfield, who appeared before us as an expert witness, threw out the hypothesis that the lower acid resistance of the chrysotile might help to explain the extent to which some studies seem to indicate that chrysotile fibers did not have as

DR. DUPRE: (cont'd.) harmful effects as other fibers have...which presumably have much higher degrees of acid resistance.

THE WITNESS: It's a perfectly reasonable hypothesis. Which are the studies, I wonder, that he was referring to that imply that chrysotile has less harmful effects for the same...one would want...when you say less harmful effects, you would want all the other variables controlled. I wonder which studies he would have been referring to. I would be interested to get...

DR. DUPRE: Well, we'll bear in mind at this point that you have a nonexpert asking you a question on the basis of expert testimony, so we are already in some degree of dilution.

But could I simply ask you this, in terms of what you were telling us about microphages...

MR. LASKIN: Macrophages. You like Dr. Uffen's word.

DR. DUPRE: Do macrophages, when they engulf the fibers, secrete acid of some kind or another?

THE WITNESS: They certainly use...well, certainly macrophages...I have to, please, I have to have you understand I'm not an expert on macrophages. It's all relative and there are other experts here, but macrophages contain lysozymes, or little enzyme containers which are their strength, and the variety of enzymes that they contain is enormous. It's quite beyond my comprehension. As one, a clinician, and two, an epidemiologist, and three, this is an area that came up long since I went to medical school. It's a very hot area of development.

However, they contain an enormous storehouse of extraordinarily powerful enzymes. Now, what and which apply to the treatment of the chrysotile fiber, I don't know. It may well be known.

DR. MUSTARD: Can I pose Dr. Dupre's question in a slightly different way?

DR. DUPRE: I wish you would, Dr. Mustard.

DR. MUSTARD: This may be some embellishment. Normally when cells...plymorphic leukocytes or macrophages...interact with particles, they take it inside and form a vacuum into which they dump the enzymes, and it's PHO, making it acid, which is, of course, the activity at which these enzymes can alterate which would fit this theory that he is proposing that chrysotile does tend to break up in this acid, or you tend to lose the fibers.

THE WITNESS: Yes.

DR. MUSTARD: The problem one would have with this, I would think, would be that if, however, the cells cannot ingest the fiber, they do battle with it, they can also discharge the enzyme into the extracellular environment, and the problem there would be as to whether the PH would drop low enough to be acid, and I guess the question I wonder, has anybody in the lung field done measurements of certain battlegrounds...it would be my area of interest just to know, in around this problem because you can get confusion into it, and I suppose that one of the questions would be, can you get..could you get this kind of attack on chrysotile fibers by enzymes released from the cells into the tissue? And you have to suppose that you can get environments where the PH of the lung drops. Do you know if anbyody has done any studies?

THE WITNESS: No, the two points that are worth commenting on: Firstly, it seems that the ferruginous body takes more than one macrophage, so that your hypothesis that the small fiber is engulfed and treated as the Chairman suggests, could well be a valid one. I don't know of work on it.

Secondly, the...particularly in France...certain studies have been directed towards analyzing lung lavage material, with a view to counting cells, to looking at the quality of cells,

THE WITNESS: (cont'd.) both macrophages and white cells, and to analyzing the enzymes both in the lavage fluid and the enzyme packages in the lavaged macrophages. I'm not familiar enough with all their implications. I do know, and I can direct you to the references, where this work is reported.

It is difficult to know quite how to interpret it clinically. You obviously don't do a bronchiole alveolar lavage without some good reason. There is evidence...there are lavages done on exposed individuals and exposed individuals with disease, and of course the presence of disease might change the whole pattern of response.

I think the payoff in this area will come in the animal work, because the elegance with which the animal studies are being pursued is, to my mind, really exciting, interesting and exhilarating...that the tiny animals are being studied with controlled exposure doses, with regular bronchiole lavages, with appropriate sacrificing to get different stages of response. Brody and Lamont - the sheep is the animal model being used in University of Sherbrooke, Quebec. Regularly monthly bronchiole alveolar lavages are being done in relation to exposure.

I believe the answer to your question will come out of this kind of study, within the next couple of years.

DR. UFFEN: Having gone from such an erudite, that brings us back down to what is lung juice?

THE WITNESS: I don't know.

DR. UFFEN: You referred to it in your paper, your review paper.

THE WITNESS: Could you give me the page, please?

DR. UFFEN: Page 200, the first line, of your tab seven. I just don't want to use the expression if it's not a proper one to use.

THE WITNESS: Well...

DR. UFFEN: My real reason is, can you measure its PH?

THE WITNESS: I think if you just...which point... could you give me the paragraph and the column, please?

DR. UFFEN: The very first line in the left column.

THE WITNESS: Oh. That refers to the fact that the material extruded from autopsy lungs, and it must contain all sorts of material. It would contain some blood, it would contain some lymph, it would contain that material that was discharged into the alveolar spaces. This section was written, and is now interestingly enough very out of date, because the field has moved very fast in this area...the thrust at this point in time was, what was the significance of an asbestos body and why was it that it was seen it was possible to recover these bodies from the lungs of individuals who had had no known occupational exposure.

It is becoming clear that we store fibers in our lungs, all of us who live in cities, and more or less fibers can be recovered.

What is of greater interest is the fact that the coated fibers represent such a small population of the fibers we have in our lungs. Anything from one to perhaps ten or thirty percent, depending on which author you read, of the fibers we have in our lungs may be coated, and the other ninety-nine to seventy percent are uncoated and lying free in the lungs. That has been realized now that the electron microscope has been brought to bear on this, and we are seeing fibers of a much smaller dimension than were ever seen five years ago.

So the interest in lung juice was just in this era when people were trying to understand the significance of finding asbestos...or ferruginous bodies in the lungs of city dwellers. But I don't think people examine lung juice anymore.

MR. LASKIN: Q. Sorry, Dr. Becklake. I was just going to ask one more nonexpert question before the lunch hour, and just following something that the Chairman had asked... back on page 196, if your suggestion that...or if the suggestion

Q. (cont'd.) that the coating of a fiber may make it nonactive is true, and if it's also true that there are more amphibole fibers that are coated than chrysotile fibers, are those two facts necessarily...

A. Compatible.

Q. ...inconsistent with the proposition that amphiboles, nonetheless, may be relatively more hazardous?

A. I don't think they are inconsistent with each other, nor with the second proposition. It is, I think, that the dust load is very inadequately reflected by the coated fiber.

As I was trying to make in the last point I made, the dust burden of the lung is very poorly reflected in the number of coated fibers and there may be only one percent of fibers that are coated and you maybe have ninety-nine percent of fibers that are uncoated, and so you have no feeling from the number of coated fibers what is the population of uncoated fibers.

Indeed, one sees this occasionally when you try to explain, to find an explanation for a case. Let me cite an example. We were sent a case of a young boy of twenty-eight who had a pleural effusion, who had had a remote asbestos exposure but he hadn't a single coated fiber in the biopsy specimen of his lung. The pathologist asked the question, could this pleural effusion be related to his brief asbestos exposure on three years at a summer job ten years previously.

Well, our pathologist was able to find a large number of uncoated fibers, but no single coated fiber. So I don't think coated fibers are useful these days other than to point attention to the previous presence, probably, of exposure if they are there in any numbers, and to make you go and look for the uncoated fibers and to characterize the exposure.

They are a marker, but they are not in any way a scientific marker of the lung dust content.

Q. In layman's terms, does your comment also suggest that macrophages as a defence mechanism are not terribly successful in deactivating asbestos fibers in the lung?

A. I have always had the...I am deeply impressed by the lung's defence mechanisms. I think it's a matter of dose, probably. I imagine there is a limit to the load that they can face in terms of exposure, but in fact if you want an example of their extraordinary efficacity, both macrophages and white cells, you only have to look at the x-ray of a lung with lobar pneumonia where you've got one whole lobe of the lung solid with inflammation. And this is perfectly capable of being completely cleared by the lung, and that particular lobe is perfectly capable of returning to normal x-ray, and all sorts of other functions. So the defence mechanisms are strong, not necessarily as strong against all agents, and obviously related to dose, I think.

MR. LASKIN: Thanks, Dr. Becklake. We should give you a luncheon break.

DR. DUPRE: Shall we rise until, say, 2:20?

MR. LASKIN: Sure.

THE INQUIRY RECESSED

\- - - - - - - - - - - - - -

THE INQUIRY RESUMED

DR. DUPRE: Counsel?

MR. LASKIN: Thank you, Mr. Chairman.

MR. LASKIN: Q. Dr. Becklake, at the risk of confusing the record even more, let me, if I might for just a moment, come back to your tab seventeen, which was the matter you started with this morning.

Your ten hundred and fifteen miners that you examined in 1967/68, and first of all I take it that those were all persons who were then employed...

THE WITNESS: A. Yes.

Q. ...in the workplace and between 1967 and 1968, and when you subsequently examined them in 1974, some continued to work and some left the workplace?

A. Yes, that's correct.

Q. In respect of any further exposure after 1967/68, whatever that may have been, that was not taken account of?

A. No, that was not taken account of in the analysis.

Q. All right. Now, as I understand it, really what you are trying to do was to look at whether there was a relationship between dose, cummulative dose and subsequent changes. That was a primary objective?

A. That is correct, particularly in relation to attacks of disease since in the prevalent study or the cross-sectional study done in 1967/68, there had been a clear relationship of abnormality to the cummulative dose when subjects were grouped according to dose. So we had fully anticipated that it was likely that those who had had the heaviest exposure would be those in whom abnormality developed.

Q. I take it you had various dust categories, and I don't know whether there is...

A. They are shown on the figure.

Q. On the figure?

A. Yes. Which are the same dust categories as previously used except that the top three are put together under one dose - two hundred-plus million particles per cubic foot years.

Q. Within each dust category, I take it, there were various employees of varying ages?

A. Yes.

Q. All of whom would have had the particular dust dosage that was in that category?

A. That's right.

Q. Then, as I understand it, what you did in order to see what the effect of dust was, you attempted to standardize those age differences across the population of workers that you had looked at?

A. Yes, exactly. If you had looked at the age distribution you would have found that the individuals in the less than ten million particles per cubic foot were, on the whole, younger than the men in the eight hundred-plus dust index group, and in order to take into account age, we therefore reconstituted each group, each exposure group, to a common age constitution, and the results you see here are expressed for a common age constitution and a common smoking profile...a simple smoking profile into smokers and nonsmokers, but nothing more detailed than that.

Q. Just to go slowly here so I make sure I understand it, in each particular dust category you had varying workers of varying ages?

A. Yes, and varying smoking habits.

Q. And varying smoking habits.

A. But they had in common the dust category.

Q. They had in common the dust category?

A. Yes.

Q. Then how would you take those calculations which you had and reconstitute them in the different way that you did? What in fact is the process that you do?

A. Well, the process goes something like this: If you look at the first group of those eligible for attack by the symptom of breathlessness, in the two hundred-plus group you will see that thirty percent...sorry, about twenty-five percent developed definite attacks there.

Of those men in that group, the numbers of whom are shown in the second table previously, there were a hundred and eighty-one men eligible for attack - table two, and the

A. (cont'd.) numbers eligibile...

Q. Yes.

A. Of whom nineteen percent had definite attacks, and forty-three percent all attacks - doubtful plus definite.

Now, those one eighty-one were broken down into age strata. I think it was by five years or it may have been by ten years, I can't quite recall. For each age strata the percentage was calculated, so that we now have this group broken down by age strata.

Q. Instead of by dust category?

A. No, within a single dust category we have it broken down by age strata.

Q. I see.

A. Now we say, now that we know the percentages by age, we can say what would have been the percentages in that dust category had the age strata looked like this: and like this was the overall age strata of the total population.

We were able to reconstitute for each dust group. We were able to reconstitute it into the same age strata.

Q. As for the whole population?

A. As for the whole population, and then back calculate what would have been the prevalence abnormality in that dust group had it had this age constitution. In this way you can compare across dust categories, taking out the factor of age.

Q. So that when you reconstitute it, every particular age category had the same...every particular dust category had the same age structure, and that age structure was the age structure of the population as a whole, that you were looking at?

A. Right. And the same was done for smoking.

Q. When you did that, I take it you found ...

A. When we did that, we found the graph which

A. (cont'd.) is...the graphs which are shown on figures one and two. These are the corrected prevalences. Prevalences by dust corrected for age and smoking differences between the dust categories.

This is fairly standard epidemiological procedure.

Q. The fact that the black lines are relatively horizontal then, in pictorial terms, suggests that cummulative dose does not significantly...

A. Didn't explain why one man got attacked and why one didn't in this population...didn't offer a reasonable explanation of the between-individual differences.

Q. What significance do you place on that finding?

A. You can conclude that our measurements of attack were inappropriate or inaccurate. We think that's less likely than that our measurements of dust were inappropriate or inaccurate, for the reasons that I mentioned and showed on the slide this morning.

The one which I think provides a reasonable explanation is that, taking into account the fact that age was an important explanatory variable in this study, is that as the exposure levels have diminished in this work force, age has become increasingly a good reflection of what happens to them in the past. The older men were there when those heavy dust exposures existed, so that what we may be seeing in the age relationship really reflects past exposure.

Q. That was going to be my next question, and that particular factor may or may not...it's not specifically excluded when you did this analysis by this design?

A. No, no. It's a reasonable explanation of the findings.

DR. DUPRE: In a sense then, age becomes a proxy for dose?

THE WITNESS: Yes. In this work force, with this

THE WITNESS: (cont'd.) exposure profile.

It may be. I don't say it does, but it's compatible with that possibility.

MR. LASKIN: Q. Should we place any significance on the fact that whatever exposure there was between 1967/68 and 1974 was not taken into account?

THE WITNESS: A. It's certainly a weakness, and the reason that we don't believe it is the explanation of our failure to show a relationship to age was implicit in...

Q. To dose?

A. I'm sorry, to dose. Was implicit in the slide showing the changing profile of exposure in the Quebec mines. What would have been added, even at the highest level in those seven years, was perhaps maybe twenty-five, thirty million particles per cubic foot years, at the most, which wouldn't have a great effect in the place these men held already on the exposure scale.

But it's relative.

DR. DUPRE: Dr. Becklake, just to go back to my question, one possibility is that age is a proxy...

THE WITNESS: Yes.

DR. DUPRE: An alternative possibility would be that older individuals are more susceptible than younger individuals?

THE WITNESS: Yes.

DR. DUPRE: If you had to flip a coin between those two, Dr. Becklake, could you...

THE WITNESS: The second possibility is an interesting one. The animal work is interesting because it suggests that younger animals are less susceptible to dust, and of course in animal experiments you can control dust. The reference I am referring to is a paper by Hyatt on Experimental Asbestosis, in the British Journal of Industrial Medicine, within the last two years.

THE WITNESS: (cont'd.) Similar data has been shown in animals exposed to coal dust. I don't know of any data in there, epidemiological data, that would go along with this possibility. I don't recall any offhand, at any rate, but...

DR. DUPRE: When you are saying young animals are less susceptible to dust, are these very young animals?

THE WITNESS: No. The study I am referring to was kind of...

DR. DUPRE: Adult animals?

THE WITNESS: Teenager animals, I think. I am not too clear about the relative ages of rats. I forget...I think this was...maybe it was guinea pig experimentation...but I remember it wasn't very young infant animals, but animals that were not very different in weight from what were considered adult animals. So I consider them teenaged animals.

DR. DUPRE: I asked that question, you realize, because among the many tails that we are chasing here is the question of susceptibility of very young humans to dust exposure.

THE WITNESS: I think the evidence on the dust for fibrosis, the animal evidence, would be the opposite. That the younger are less likely to respond to a given dose. But it's very slender evidence and I'm not aware of any evidence in man. I think the question is a very interesting and an important one which I would like to see addressed.

DR. DUPRE: But in any event, the young animals you would characterize as young teenaged animals, for lack of a better term?

THE WITNESS: Yes.

MR. LASKIN: Q. Can I ask you more generally in respect of these various changes that are occurring, and to what extent is it possible that any of these changes could be attributed to the dust itself, rather than whatever asbestos fibers there are found in the dust?

THE WITNESS: A. It's certainly very possible. I rather favour the view which has been expressed in terms of gold miners, and I think also in terms of...gold miners in South Africa...and I think also in terms of coal miners elsewhere, that miners at any rate probably suffer from more than one occupational disease which is related to exposure, that the rock dust itself and its heavy deposition in major airways may well evoke a chronic bronchitis that's an airway disease.

In addition, that is a different sort of disease affecting different parts of the lungs and possibly related to a different sized particle. But linked...on the other hand there is the disease which is attributed to the asbestos fiber, which is by nature a scarring of the lung, also dust-related, affecting different lung structures, also related to exposure. And it's quite possible that we are seeing a combination of these effects in our studies.

The only changes which are likely to be more specific for fibrosis are, of course, the parenchymal radiological changes. But breathlessness, obviously, is a nonspecific symptom. Forced vital capacity is something that possibly relates to scarring more likely than to airway disease, whereas maximum mid-expiratory flow rate might well relate to airway abnormality rather than to parenchymal abnormality.

Q. Can I take you back to your New York Academy paper, which is at tab eleven? Page 24, right at the top you refer to a matter which is of some interest to this Commission and which we've had some evidence on, and that is your observation...and I take it here you are looking to the Quebec cohort, the mortality study.

A. Mmm-hmm.

Q. Your observation that there were also deaths, although not attributed directly to pneumoconiosis, that may well

Q. (cont'd.) have been the consequence of fibrosis due to dust retention. Are you with me?

A. Yes, I am with you. It's Dr. McDonald's phrase.

Q. Which one?

A. J.C.

Q. Which phrase?

A. That one about deaths, although not attributed directly to pneumoconiosis may well have been the consequence of fibrosis due to dust retention.

It comes from the mortality paper, the 1971 mortality paper, I think it was, in which the deaths attributable to pneumoconiosis were identified, and if you will recall they were particularly heavy. I think a relative risk of twenty-eight in the very heaviest exposure group, and a much lower relative risk in the lower dust groups.

Q. Can I ask you generally or specifically with reference to this cohort what kinds of other causes of deaths may be contributed to by fibrosis or asbestosis during life?

A. Other respiratory diseases, which would include things like pneumonia. Tuberculosis may have been important in the early days of the cohort but was probably not important in the latter part of the followup.

If you will recall the first slide I showed you, which was the table on prevalence in the review paper...I forget which tab number that is...

Q. Seven.

A. ...even for the highest exposure groups when deaths in the first followup were of the order of thirty per thousand men at risk compared to ten per thousand men at risk in the lowest exposure group, and that was deaths for cancer, you will recall that heart disease deaths were, as in all populations of men of this age, very much more important as a cause of death. I think it was a hundred and twenty per thousand. They vary between a hundred and twenty and a hundred and forty

A. (cont'd.) across the different dose categories.

I don't recall in my head what the updated figure in the subsequent followup study, or the subsequent reanalysis of the cohort, is deaths accummulated.

The issue of whether deaths from heart disease, in particular corpulmonale, is an important factor in this group of men also comes up. As to whether pneumoconiosis might put strain on the right heart and cause death for that reason also comes up as a possibility. I don't think we have any further evidence to clarify that.

There is, by analogy, quite good descriptive evidence of a clinical nature from the Finnish workers...I could find the reference, the paper I'm thinking of...on electrocardiographic changes indicating right heart strain in asbestosis, and there is a high prevalence of changes suggesting heart strain and therefore making more likely the possibility that the right heart might fail in the presence of asbestosis and other stresses.

Q. Are you suggesting that the medical evidence to date hasn't finally settled that question as to whether one may cause the other?

A. Oh, I think there is no doubt in general terms that lung disease can cause right heart strain. Certainly asbestos lung disease can cause right heart strain if it's severe and advanced.

How often it happens to have been the case in this series is the matter on which I am less certain. It certainly can do it. I think that that's classic textbook evidence which I think would be generally accepted.

Q. Is it fair to say that today we are seeing considerably less what one may term clinically-disabling asbestosis than in the past? In other words, as standards seem to become more stringent, can we say one way or the other whether we are

Q. (cont'd.) seeing relatively more or less disabling asbestosis?

A. One sincerely hopes so. I am not aware of good evidence on which to answer that point. Either in the Quebec series...for instance, is there evidence that the number of cases requesting and receiving compensation is diminishing?

I'm not aware of it. It could be there. I haven't had access to or analyzed the compensation data. Nor am I aware of any study to date which has consistently answered the question as to whether the implementing of controls and therefore lowering the pollution levels to which individuals are exposed has had the expected, hoped for effect in improvement of health.

I believe the data might be there. I think it might be so analyzed. I am aware of a study in Newfoundland, in the Baie Verte mining area, which is hoping to address this very issue.

Q. Done by whom?

A. The Workmen's Compensation Board in Newfoundland. It's a population, also, which has been studied by the Mount Sinai group. I understand they may be going back to do a repeat study this year.

The interest in that work force is that it is a work force that has, as far as we know, been exposed to environmental levels which have been far more in conformity with standards than have any other work force, as far as I know. I believe the study of that work force is extremely important in terms of providing evidence on whether or not that type of environmental control has produced the expected and hoped for effects.

Q. Your mention of compensation leads me perhaps to ask you one or two questions about that, from a medical point

Q. (cont'd.) of view.

One of the categories of compensation that we have in Ontario is something called asbestos fiber dust effects. Does that have a medical significance to you?

A. I imagine all the things we are talking about are asbestos fiber dust effects - fibrosis of the lungs and pleura. I presume that is what it refers to, is it?

Q. It is my understanding...

A. Is it defined...

Q. As I understand it, it's something different from asbestosis, I suppose, something loosely called preasbestosis.

A. I'm not familiar with that definition.

Q. No?

A. Can you tell me how it's diagnosed?

Q. I would be treading on ground, Dr. Becklake, that I am probably very inexpert to tell you, but perhaps we should just leave it at that.

A. Well, I imagine what is referred to is radiological changes that are not definite...radiological changes have been generally used as probably one of the more objective indices of abnormality. The difference between what is normal and what is not, or what is not normal...especially at the lower end of the scale, is extremely difficult to determine because it is influenced by the technique of the film taking, by the technique of reading, and by whether the individual has taken a deep breath or not, how closely he stood to the x-ray film.

So that whereas nobody has difficulty in diagnosing definite effects, nearly everybody has difficulty in diagnosing... and I say in quotation marks...'early effects', radiological or whatever the early effects are.

Q. On that question, does your...is it your judgement that the most sensitive detector of early effects is lung function changes?

A. I think that the evidence suggested in some individuals the first changes appear to be in the lung function, in other individuals the first changes appear to be on x-ray. I believe it is unlikely that there is a simple answer to that question, and indeed it would be very unlikely if lung function always was affected before x-ray or the alternative.

Q. In light of the answer to that question, do you have any judgement on what kinds of examinations may or may not be appropriate for workers exposed to asbestos?

A. I believe they should include clinical evaluation, a lung function measurement and an x-ray examination.

Q. How often?

A. I don't believe I can give an answer to that. How often...I don't know of any good evidence on it. It is customary in some legislations to call for them pre-employment, within a year and then within five, or six or seven years, and then after a ten-year exposure, annually. That is the type of approach taken with the silicosis legislation, for instance, in South Africa.

The other approach is, of course, the annual x-ray. I believe the annual x-ray was particularly important, particularly in the early stages of exposure when TB was a real complication. As TB has become less important, one wonders if the annual x-ray, certainly within the first few years of exposure, perhaps doesn't offer more exposure to x-rays than is necessary in terms of the yield.

I don't believe the answer to this can be made on scientific grounds. I think it's a judgement, and indeed I think probably the important part of the examination is that the worker who is being examined feels that his health is being seriously surveyed.

Q. Can I just come back for one moment to the

Q. (cont'd.) medical side of that compensation issue, and I notice that throughout some of your articles you commented on that issue, raised it. In this jurisdiction, in order to compensate for asbestos-related disease, mesothelioma and lung cancer, there are generally two components. One component is a time period of continuous exposure, generally a minimum of ten years. The second component is a minimum period from time of first exposure to time of onset of the disease.

My question really is, in your capacity as a physician and having observed persons with asbestos-related diseases, is that kind of approach a sensible approach?

A. I think it's...what you mean is that there are certain rules set about what is the minimum exposure. I would doubt if those rules have scientific support. I would think that there are undoubtedly cases attributable to exposure that don't match those rules.

I think the...certainly for mesothelioma the evidence suggests, from all the..what do you call it...all the domestic exposures, this is not continuous exposure and it's not continuous for ten years. So I think there's one example of where that would not take into account...that would not correctly attribute an illness if that was the requirement to make it attributable.

Q. Are you suggesting from your experience that it's not entirely appropriate to have specific kinds of rules to apply?

A. The difficulty about making rules such as you suggest is that you will undoubtedly be put out of date quite quickly, and that you will undoubtedly not correctly attribute certain cases. As I said, I think the judgement of the attributability is best done as other clinical judgements, taking all factors into account - heaviness of exposure, nature of exposure, nature of the fiber concerned, proximity to the

A. (cont'd.) exposure. I believe that it would be...if I was writing the laws I would write them in such a way that one could use judgement as you make other judgements, and rather than precise the rules under which circumstances you would admit a given case as being attributable to a given exposure, my clinical experience...which is not very large in handling individual cases...would tell me I would be made a fool of too often to be comfortable with those kind of rules.

DR. DUPRE: Just a quick question, Dr. Becklake were you involved in Dr. McDonald's study of the Canadian gas mask workers?

THE WITNESS: No. I know about the study and the time periods...

DR. DUPRE: That was a very short exposure.

THE WITNESS: A very short exposure, exactly. And probably under very...and that's one of a number of studies which makes me think that certainly for mesothelioma such a rule or such a restriction as to what constitutes an exposure to which you might attribute the disease would be incorrect, to say the least of it.

MR. LASKIN: Q. Would you extend the same reasoning to, for example, lung cancer or even gastrointestinal cancer?

THE WITNESS: A. I think lung cancer is a much more difficult issue because it is a common disease in the general population, as you know, quite independent of asbestos exposure and the issue is a much more difficult one.

Are there such rules written in the legislation in Ontario?

Q. Yes.

A. The same figures?

Q. My recollection is that, but I stand to be corrected.

Q. (cont'd.) Let me try another proposition. It was one put by Dr. Weill when he was here, which related to that.

As I understand it, his proposition was that roughly speaking a fibrogenic dose of asbestos will be equivalent to a carcinogenic dose, and leaving aside for the moment whether there is any medical support for some causal relationship, if you have one, you are likely to have the other. As I understood it, he put forward that proposition from two points of view...one from the individual compensation point of view, and secondly, from the more general point of view of trying to establish an appropriate standard for controlling asbestos.

Can I ask you, in your experience as a clinician, as a medical doctor, whether in those cases that you find, for example, lung cancer after asbestos exposure, you also find some evidence of fibrosis?

A. This is a difficult issue. I would like to make one or two points. I'm interested that Dr. Weill offered the view that a fibrogenic dose was likely to be a carcinogenic dose.

Am I correctly interpreting what you said?

MR. WARREN: I think that's not quite right.

MR. LASKIN: Q. I think what he said was that in order...let me try it again...from the individual point of view, perhaps more correctly as I understood it, his thesis was that in order to decide whether you attribute any particular lung cancer to asbestos exposure, you look. And if there is evidence of fibrosis, it is a good guideline that you can attribute that particular lung cancer to asbestos exposure.

THE WITNESS: A. I think that's the guideline on which compensation boards operate, whether it's a written guideline or whether it's an implicit guideline in their behaviour. I think that is how they operate, yes.

Q. In your own experience as a medical doctor, does it seem to you to be a reasonable guideline?

A. I can't talk from my experience as a medical doctor because I don't see a lot of cases individually. I would be prepared to offer a thoroughly speculative comment on interpreting the medical literature as I see it.

Q. Please do.

A. I think the evidence suggests that, oh, you may refer to Kannerstein's article at the more recent Lyon meeting in 1979, in which he attempted to address this issue indirectly.

He analyzed the cell types of lung cancers seen in relation to exposure to asbestos and compared them with cell types seen in nonexposed populations. Some studies have suggested that the asbestos-exposed individuals have more adenocarcinoma, peripheral type lung cancers, which are by analogy to other types of fibrous lung disease thought to be...and I put in parenthesis...scar cancers.

On the other hand, other series have shown the squamous cell cancer...or the distribution of cell types in asbestos-exposed populations to be similar to the general population. So there is conflicting evidence on that.

There is the other interesting issue that in the general population adenocarcinoma is becoming the more common of the lung cancers compared to what it was five or ten years ago. Again, I quote Dr. Kannerstein's review on this.

My own interpretation of the evidence is that there may well be two different cell types of cancer attributable to asbestos...one which relates to squamous cell cancer in the major airways, and one which is indeed a true scar cancer, and therefore likely to be related to or stimulated by the presence of fibrosis in the lung.

A. (cont'd.) It's interesting that the paper of Dr. Liddle's in the recent British Journal of Industrial Medicine would go along with that view.

He looked at the usefulness of radiological abnormality in predicting outcome in this same famous thousand and fifteen men, plus various others from the mortality cohort. He found that radiological abnormality was a good predictor of a bad outcome - namely death.

Secondly, that implying then that there was radiological abnormality and therefore some fibrosis and that predicted outcome - in other words, death by cancer of the lung, but not in all cases. There were some cases in which the death was attributable to cancer of the lung without evidence of scarring.

So I think the evidence can be interpreted as compatible with the possibility that there is more than one cell type attributable, one which might be linked to scar cancers and one which might be from some other interreactive mechanism and primarily concerned with...primarily of the nature of squamous cell cancer in major airways.

Q. That other type that isn't the scar type cancer may still, I take it from what you said, to be related to asbestos exposure?

A. Yes. I think that's very speculative and it's interpretation of difficult data which is really perhaps overinterpretation, if you like, of data which is not clear. But that, I think, is a possible explanation.

Q. Can I ask you just a few questions about pleural changes? If they occur by themselves without any parenchymal changes, are they associated with clinical or functional disability, or are they generally relatively benign?

A. The changes referred to as pleural plaques, which are localized areas of fibrosis of the...usually parietal

A. (cont'd.) pleura, but more rarely the visceral pleura...are usually without clinical impact. In other words, they don't produce symptoms, they produce an x-ray abnormality.

In epidemiological studies you can show they have an effect on function in the sense that if you...our own data suggests this and other peoples' data suggests the same thing.. that if you take individuals with normal parenchyma, who have...now let me put it another way.

If we looked at the influence of the lung function in individuals with similar parenchymal changes, those who had pleural abnormality had slightly worse functions than those who had no pleural abnormality, and that went from the range of no parenchymal disease to quite marked parenchymal disease.

But that's on the basis of epidemiological studies. In the individual it would be very hard to pick up an abnormality of function because your interpretation depends on referring to some normal standards, and as I've mentioned before the reserve function of the lung is so enormous that you have to lose a lot of function before it's possible to say that a change is definitely a limitation or diminution of reserve function.

There are, however, well documented cases, particularly in the shipbuilding industry, of rather rapid, rather progressive, rather severe pleural changes. They have a very important impact on function, and they have a different kind of prognosis.

Q. They are asbestos-exposure related?

A. Yes. They can become extremely severe. They can block out the whole pleural space on both sides. They can lead to right heart failure and really they are as severe as that, but this sequence of events was well documented in the prospectus studies of Shears and others in the British naval dockyards.

A. (cont'd.) I'm not aware of it in the Quebec mining exposures, and I'm not aware of it being described in other exposed populations, but it's possible that it has not been looked for specifically.

Q. Can I ask you whether your own studies have gone beyond the occupational setting into the general environment or general public setting?

A. Not in terms of asbestos, no. No, we haven't.

Q. From your own research and from your own occupational studies, do you have any judgement on what kind of health risk the public is looking at from...I put that in its most general sense...from asbestos exposure?

A. As regards...in the first place it has been surprising to me with the evidence that environmental pollution outside the workplace was quite high in the two asbestos mining towns of Quebec, in Thetford Mines more than Asbestos, and in view of the high fiber counts in the water supply of Thetford Mines, it is surprising to me that there has...that health effects in the general population have not been easier to demonstrate.

There has been one mortality study done by the Quebec government, the Department of Epidemiology, and I think there are one or two other studies that I can't recall offhand. But in that it has been extraordinary to me that there has not been...it has not been possible to demonstrate an impact on the community. It is possible that the studies, there were defects in the studies, I don't know. But it is surprising, knowing the situation as it is now, that it hasn't been readily attacked and demonstrated.

The risk to the general population living in urban areas, I have not reviewed with any great care, I have not reviewed the risk in public buildings. I had hoped or had looked to the report of the proceedings of this Commission as offering an extremely sound review of this data, which I

A. (cont'd.) believe to be needed for general reference, and I had hoped that this would be one of the outcomes of this Commission...the medical data would be carefully, scientifically reviewed and this would be a reference to everybody in this area.

Q. Let me just pursue one other line of questions with you, and it relates back to this line about questions early detection of evidence of asbestos.

We've had some evidence before us that the British...and I'm sure you are aware of it...when it chose to establish a standard for asbestos in 1968, chose to take as its index a morbidity index, evidence of rales or crepitations, and then make some assumptions based on that.

My question to you is, do you have any reaction or judgement as to the sense in taking the morbidity index as opposed to a mortality index for the purpose of developing a control standard?

A. No. I think I haven't any comment to make on that. It has been the custom to use the morbidity index. I refer back to an earlier question, when you said you were quoting Weill on this issue of whether a common dose, if it produces ...or if you are protected against asbestosis are you also protected against cancer. I'm interested that he made that point because I had interpreted the historical sequence of events that the first asbestos-related disease to be described was a fibrosis, and that was described around the turn of the century, in the early part of the century, when exposure levels were really extremely high and individuals died off before they got to the cancer age group, that the link in association with cancer was noted first in the thirties and suspected in the thirties, and as people lived longer with their fibrosis, they entered the cancer age group. So in a sense if you are protected against fibrosis I guess it's reasonable on that evidence to

A. (cont'd.) assume that you might be protected against cancer, too. But I don't know.

MR. LASKIN: I think, Mr. Chairman, I should let my friends have a chance to question Dr. Becklake, who has been most patient with me.

DR. DUPRE: Thank you, counsel.

Do you have an agreed upon order?

MR. WARREN: Really, I don't think we do, as a matter of fact.

DR. DUPRE: Would you like five minutes?

MR. WARREN: Surely.

DR. DUPRE: Or do you simply wish to have one of you by default?

MR. WARREN: The three of us, apparently, have to decide among ourselves.

Shall we take five minutes?

DR. DUPRE: Yes. Shall we reconvene about three-thirty?

THE INQUIRY RECESSED

\- - - - - - - - - - - - - -

THE INQUIRY RESUMED

DR. DUPRE: Have we a batting order?

MR. WARREN: Yes. One, two, three.

DR. DUPRE: Miss Jolley?

MR. McNAMEE: I'm going first. I have no questions, Doctor, thank you very much, and Miss Jolley will now take the floor.

CROSS-EXAMINATION BY MISS JOLLEY

Q. Dr. Becklake, I would like to go back to figures one and two in tab number seventeen again, and I think that...I just want to go over one of the comments that you made

Q. (cont'd.) about those figures, and that was that...and I wonder if you could explain to me...you said something about the measurements perhaps being inappropriate, and I just wonder, could you just elaborate on that?

A. Breathlessness, abnormalities in forced vital capacity and maximum midexpiratory flow rate are all measurements or all abnormalities that can develop in response to a lot of exposure to many agents, known and unknown.

Q. I think I was more interested in your comments on the dose measurements, the fact that...

A. Oh, the dose measurements, right.

Q. ...they are not related to dose.

A. You don't mean response measurements.

Q. No, I'm sorry.

A. The dose measurements, yes, I suppose the first point I made was that in the measurement of dose we used for this analysis was the same cummulative exposure index which we had used in the prevalent studies which formed the subject of the first slide I showed you this morning, in which this same measurement in the same population was capable of...let me put it another way...using that measurement to classify people in exposure levels, there was a clear dose relationship or exposure-response relationship. The same measurement of exposure - same population.

Now when we come to ask the question, did that measurement relate to changes over the next seven years, we find a much less clear relationship, and my question was then what factors, why is this relationship no longer reflecting change as we would have anticipated on our original studies.

Of course, I can...we can find far more, many more reasons why it shouldn't relate to change than it should, because it is such a poor, if you like, expression of exposure over time, and then I listed the various reasons why. It is an exposure index based on particles, not fibers that are thought

A. (cont'd.) to be important. It is based on area sample, not personnel sampling - so much less likely to describe an individual's exposure. It has not taken account of particle size, and the respirability of dust is important to its penetration and retention in the lungs.

It takes no account of the profile of exposures, so a long, low exposure is equivalent to a short, peak exposure and this is unlikely to be the case.

So that there are many reasons why this exposure index...many reasons why it would be a poor indicator of the exposure that is relevant to progression or attack by these features.

Q. Would the same comments be made on the exposure levels in the mortality studies of the chrysotile miners this way?

A. Oh, yes. You could make the same comments in relation to all of them. But in all those instances, a strong relationship to exposure was shown.

Poor as it is, this dust index, this cummulative index appears to be something that reflects whatever it is at risk in the working environment that produces a bad outcome.

All of them, obviously, are approximations or proxies, as the Chairman said, of occupational exposure...and bad proxies at that.

Q. Thank you.

MISS JOLLEY: I would like to distribute a letter that you wrote to Dr. Cameron Gray, in 1974, and this is part of the Workmen's Compensation Board's submission to the Commission.

THE WITNESS: Can I have a copy?

MISS JOLLEY: Yes, sorry.

MISS JOLLEY: Q. I think Dr. Gray asked you to address a number of questions relating to Workmen's Compensation over asbestos disease in Ontario, and you very kindly responded.

Q. (cont'd.) In light of your comments today about the bronchitic responses...which I found very interesting because few other researchers have presented that kind of data to us...I would like to refer you to page two, and I'll give you a few minutes to read your response.

THE WITNESS: A. Yes, it's close to what I tried to describe today.

I think I should distinguish between what I believe and what I have evidence for. I think what we have evidence for is the obstructive function profile is common in asbestos miners and millers in Quebec, that it is particularly in those who smoke, and it is at least as common as the restrictive profile in the presence of radiological asbestos. That claim is based on an analysis of over...Dr. Fondimare's analysis of about three hundred-some published individual cases, where the obstructive profile was found not quite as commonly as the restrictive profile, but was found to be very common in individuals who had radiological evidence of fibrosis.

I don't believe we correctly understand the interrelationship of exposure to dust and smoking on the one hand, and obstructive phenomena and fibrosis on the other hand.

I think I mentioned this morning that I find quite a compelling interpretation of the data, the one offered by Dr. Weill in relation to the gold miners in South Africa, that they have an obstructive type of phenomenon which is dose related to exposure, as well as a restrictive type of phenomenon which is presumably a scarring type of fibrotic phenomenon, both being exposure related...possibly to different elements in the occupation, possibly to different types of dust or sizes of dust in the occupational exposure.

I think the Quebec data is consistent with that view, but I can't go further than that in saying that it's consistent with the view.

A. (cont'd.) It's interesting, in 1970, Dr. Gilson gave an address to the Royal Society of Medicine in Britain on Industrial Bronchitis, question mark.

I think Dr. Higgins' review of the role of industrial or of occupational exposures and their association with bronchitis, together with the paper on Industrial Bronchitis by Dr. Morgan of the University of Western Ontario, has removed the question mark from this statement.

I think many occupations are associated with an industrial bronchitis in the sense of coughing and spitting. Whether or not that is associated with airway obstruction and disability is another issue, but that's using bronchitis in terms of the definition used in epidemiological studies, which is chronic coughing sputum.

I think it deserves working on.

Q. Would you still maintain the one comment? This means, I believe, that both profiles should be accepted as associated with asbestos exposure?

A. Yes. At the gut level, yes. But I don't... that's a personal opinion and I haven't got strong evidence to support it.

Indeed, I would like to see this as the target for research work in the next few years.

One thing that I have found very interesting in the new work on the response to dust exposure at the cellular level, is that to which I referred this morning in terms of what happens when you inhale a particle or a fiber. The first response of the body is in the macrophages, and in turning on the macrophages and in calling out the other defence mechanisms.

The evidence is that cigarette smoking does that, asbestos fibers do that. I should think that the evidence is probably that other forms of dust might also do that.

I believe that there is a great deal of work to

A. (cont'd.) be done at the cellular level to clarify this relationship and response.

I don't know to what extent it's ready to be taken over into clinical action. I believe it's an area which merits and should call for research to clarify these relationships.

Q. I would like to move on to some comments made throughout your papers, and I understand that a number of the comments were made from a clinician's point of view.

I guess that we in the labour movement worry about the term susceptibility, and I wonder if you can tell me how clinicians define susceptibility now?

A. I think it's a bad word, probably, to use because it has implications of the kind that you imply. If you use it in inverted commas and say the factors that account for differences between individuals' response to apparently the same exposure, you would agree with me that individuals who appear to have exactly the same exposure...two indivuals or two groups of individuals who appear to have the same exposures, may show differences in response.

Now it's more than likely that a lot of those differences relate to how much dust they actually breathed in, that we haven't so correctly characterized their exposures, we don't know anything about their individual clearance patterns, we don't know anything about the stress under which they work, the volume of air they breathe in. We know that the harder you work, the more air you breathe in, the more dust, certainly in animal experiments, appears to be retained.

So there are many factors in which...would vary the exposure level, which is just the level in the outside air and what remains in the lung. And I think that the word susceptibility has medical connotations which make it also an unfavourable word, or a bad word, to use because it has too many other implications. I think what I wanted to draw attention to

A. (cont'd.) is that different people appear to react differently to a comparable dose.

Q. But in terms of the medical knowledge as it is now, there is nothing substantiated about...

A. No. Well...there is some evidence that certain individuals in other dust diseases, not particularly necessarily in relation to asbestos exposure, but that certain individuals who have a rheumatoid tendency, for instance, may react differently to coal dust exposures. Some people get Kaplan's syndrome.

There is evidence of some unusual factor in the host to produce a rather extraordinary reaction, so that there are analogous situations...not, to my knowledge, in relation to asbestos exposure, but in relation to other dusts, that imply that certain circumstances change an individual's susceptibility or change an individual's reactivity to a dose producing a rather more marked response than might have otherwise occurred. But it's very circumstantial evidence and is really, I think, perhaps only beginning to be explored medically.

Q. In terms of public policy, is it something you feel we should be pursuing to identify workers who should be essentially kept out?

A. I think it's the job of physicians and the medical profession to understand as completely as possible the nature of human responses to agents. But I think it's certainly nothing that could be implemented in public policy.

I think it's a medical puzzle. I believe the better we understand it, the more we will be able to protect individuals. So it certainly merits study medically.

Q. There was another comment that other expert witnesses have actually said in their writings, but perhaps I would just like to draw attention to the Commission. That was on page 218 of tab seven, and that is, when you are talking

Q. (cont'd.) about standards, you are talking about...I'm sorry...page 218 in your second paragraph...sorry.. your second...the righthand side of the page.

There is a quote: "Furthermore, because all standards should be regarded as no more than expressions of the best available hypothesis of levels adequate to protect human health, they should always be reviewed in light of subsequent evidence."

I just thought that was a very important statement because we have a tendency to state that we are going to make standards for all times, and some of our processes essentially mean that. But I think that's an excellent comment.

A. That's actually quoted from the OSHA criteria documents. They made that statement and I think it's a very important statement. Like you, I think it's a very important statement that the interface between medical science and the public, that they are no better than the best available hypothesis, and that is in fact quite clearly stated in the preface, I think, to nearly all of the criteria documents.

Q. Do you have any suggestions for how, in a standard, one could provide a mechanism for a constant review of the literature?

A. No, I'm not the expert in this area, but I agree with you that it's like all medical information, it needs to be reviewed.

MISS JOLLEY: Thank you very much, Dr. Becklake.

DR. DUPRE: M. Casgrain.

<u>CROSS-EXAMINATION BY M. CASGRAIN</u>

Q. Dr. Becklake, towards the end of your examination-in-chief, you mentioned, you stated that in your view the examination of the worker, someone who is exposed to asbestos,

Q. (cont'd.) included the following: a clinical examination, lung function tests and an x-ray. I would just like perhaps to emphasize a little bit on this and add a little more specifics.

For instance, I presume that to this you would add the occupational history? Is that correct?

A. Yes, indeed.

Q. Is it correct also...

A. No, I was asked about the clinical examination, what elements should the clinical examination or the health examination...at least I understood the question from Mr. Laskin that I was not asked how a clinician should evaluate a case, but what should be the elements of an annual examination. So that was what I was responding to.

Q. You would add to that examination the occupational history?

A. Of course, because a surveillance examination implies that there is a parallel record of the occupational exposure, indeed.

Q. Would you divide the examination of the patient, in a clinical sense, I suppose, into two phases: (a) the subjective side such as breathlessness and expectorations, as opposed to the objective side which would be the x-ray?

A. Well, it depends on what purpose I'm doing this surveillance examination for. Could you fill me in on the context in which you would like me to answer your question?

Q. What I'm trying to get at is, what in effect, in your view, would be a complete physical examination of someone working either in a factory or a mine?

A. For what purpose?

Q. For compensation.

A. Oh, compensation is another issue. There are quite different kinds of examinations. Now, that's why I thought

A. (cont'd.) perhaps there had been a misunderstandin here. There is one thing the examination that the worker undergoes once a year in a clinic or whatever, the examination which he considers is the effort on the part of whoever is responsible for his health to detect abnormalities, and presumably which he seeks because North America is hepped on the annual examination, if you like, but because we believe it protects us against something, so the worker, like anybody else, believes that such an examination is useful for maintaining his health.

There is...the epidemiologists have probably pricked that balloon and I can refer you to Dr. Spitz's paper in the Canadian Medical Association Journal.

However, that is quite different from a compensation examination, and that is another evaluation, a different kind.

Q. All right. How about a middle term? How about monitoring the workers?

A. That was the first, the examination...I was suggesting that monitoring a work force means everybody in the work force, and people who have left, get certain examinations at the same time their environment is recorded, and their exposure and their work history is recorded. So I'm assuming that that monitoring examination goes in parallel with an equally careful description of what has been that individual's exposure in the workplace in the last period of time.

Q. All right. Perhaps if we went to the monitoring examination. Would you agree that you would include in it occupational history?

A. Well, that's the monitoring system. I don't know whether the physician would ask...you are asking me should the physician ask the occupational history? I don't know what system should be set up. There are probably much easier ways to get the work history. You get it off the job description. So I

A. (cont'd.) still don't believe...you must tell me what is the objective of the monitoring examination. Different examinations have different elements because they have different purposes, and I'm not quite clear of the purpose of the examination you are asking me about.

Q. To put it very simply, Dr. Becklake, I am referring to a worker who works in a mine, say, and who undergoes his annual examination.

A. By whom?

Q. By the clinic.

A. By the clinic, yes.

Q. All right? I'm just asking you whether you agree with me that in those circumstances this examination would include an occupational history, which he would get anyway from the...

A. Sure. Absolutely.

Q. Plus the subjective steps which were the rest of those...

A. Sure.

Q. And expectoration, plus other signs...

A. Yes, it's a medical or respiratory symptom questionnaire and respiratory clinical examination and a chest x-ray.

Q. Which would include auscultation for basic rales, for instance?

A. Yes, and for wheezes and other...

Q. Yes. The whole pulmonary function test, which include vital capacity and so on, is that correct?

A. Well, if not include that, it might include more complicated tests, but some measurement of pulmonary function.

Q. At the minimum. The next question I want to ask you is the following, because you talked about...and I think you have a paper on it too, as well, but generally you spoke

Q. (cont'd.) this morning about the withdrawal of the worker.

What I would like to ask you is the following, and if you prefer not to answer the question because of lack of experience in the matter, you may say so.

Assuming you have before you, as a clinician in a clinic, a worker who would have undergone these tests, with the age, say, fifty-five, and who as a result of these tests would be found to have an incapacity of, say, ten percent incapacity.

A. Yes, that's a very...a very...I would be interested to know on what criteria this incapacity was...

Q. Could we discuss that, perhaps?

A. Okay, I would be very interested. What criteria would this incapacity have been rated on? What measurements?

Q. I was going to, perhaps...I had assumed that you would know, but I'm going to ask you what you think are the criteria for incapacity.

A. In capacity for what? You must define what..

Q. The incapacity of a worker.

A. To do what? To work at his job?

Q. Yes.

A. Well...

Q. Or generally. No, I would not say the worker. Just general incapacity as a result of the tests...

A. It's a term which has caused more misunderstanding in the medical profession than any other term. It's a term that is currently the subject of wild debate in the U.S., as you know.

The custom has developed to use the word incapacity to mean a decrease in lung function, and disability to mean inability to carry out a certain degree of job.

Now, incapacity, that means that you are precisely

A. (cont'd.) able to define what a person's normal lung function is.

Q. This is correct.

A. The major cause of difference in a measurement of lung function between individuals is size, so that a tall man has bigger values than a smaller man. In order to take size into account, because you aren't interested in differences of size, you are interested in differences due to other things, you have to take some outside reference standard into account. So you use what are prediction formulae to quantitate what is a person's lung function and does it fall within the expected range.

What formula do you use? Is it appropriate for me in Quebec, for miners in the Eastern Townships, to use formulae derived from the population derived from the population study in Tucson, Arizona, or is it more appropriate for me to take figures from the U.S. Veterans study done in twelve cities, mixed races?

I'm trying to say that the definition of abnormality or incapacity precisely is a very difficult thing to do.

Yes, when that is fifty percent below expected, yes, there would be no doubt that the grey zone is well passed. But to precisely define incapacity in terms of lung function is, I don't believe, possible in relation to the interpretation of a lung function test.

So, okay, let's then accept that it is very difficult to define what are the ranges of normal. You can define gross abnormality, but you don't need lung function tests to help you at that stage, usually.

What is disability? I believe that you cannot predict...sorry. Yes?

Q. It was because you were talking about incapacity and disability, and I think I would like to stay on

Q. (cont'd.) incapacity a little while, if you don't mind.

A. Yes.

Q. I think you know that the Quebec Workmens Compensation Commission has certain terms of reference in respect of incapacity?

A. I would be interested to have them repeated for me.

Q. I think it has to do precisely with the lung function tests and incapacity of the lung...

A. I'm not aware that they have described any accepted levels. I'm not aware that they have published levels that they use, or reference standards that they use as their standards on which to judge an individual's function.

Q. I don't believe they have published that.

A. No, I don't think they have, and I'm not aware of their practice because I don't sit on any of those Compensation Boards, so I'm not aware of their practice.... only indirectly insofar as I occasionally see a case which they have judged.

I would like to make another point though. I think that because...I think that you have to remember that the lung has an enormous reserve, that the measurements that we call lung function tests are really descriptors of lung size and lung gynmastics. They don't necessarily relate to what you can do in the way of performance.

Q. Perhaps we can take it from there. Assuming that you would have an individual who would have an x-ray reading at, say, zero slash one, using the ILO standard, and that his vital capacity, his pulmonary function test, would show, as opposed to one hundred percent on an average, say eighty percent or ninety percent. One would therefore assume that he has, theoretically, ten percent incapacity in lung function, plus

Q. (cont'd.) zero one slash. Can we take that case? Would that satisfy you?

A. Yes, except that I would like to make one point. In clinically reporting lung function tests, you would never say that you expect an individual...that you expect individuals without disease to be one hundred percent. They would range between eighty and one hundred and twenty percent because of our inability to predict precisely what an individual's normal would be. We don't have the...how do we develop these prediction formulae is simply by studying health populations without exposure, and looking at the influence of height and using the relationships generated by these populations to height-correct in our own case under discussion.

So it's a very weak tool, if you like, for precision in defining lung function.

Q. Which is why you need the other tests along with it, mainly the x-ray, the basal rales...

A. No, which is why you must do a much better study of lung function than simply a measure of vital capacity.

The point I wanted to make is that the resting lung function, vital capacity and lung gymnastics do not closely link with the performance of an individual on exercise. What's important to a man is whether he can sustain his job, which requires him to sustain an oxygen consumption above his resting level, comfortable for an eight hour, or whatever the working period is.

Now, most of us can increase our oxygen capacity by a factor of twenty or thirty. If you are a good skier and cross-country ski training, you might be able to push your lung function to do thirtyfold more on exercise from what you can at rest.

If you are like the average North American, maybe

A. (cont'd.) it's only twentyfold more. The Swedes go to thirtyfold more.

If you are like the average, out-of-condition North American, it's less than that.

Nevertheless, you have a capacity to increase the gas exchange, which is the crucial function of the lung, considerably.

Now, what can you sustain? You can't, obviously, work at maximum. You can't work at your maximum. The hockey player can do a burst of exertion, but he can keep it up for half a minute. He goes into oxygen debt and he requires a period of recovery.

What can you sustain as a sustained level above the resting comfortably for an eight hour working period? The work from the team at McMaster, and in particular Dr. Jones, have carefully quantitated exercise adaption and allow us to say that you can sustain probably sixty percent of your maximum, comfortably, for an eight hour working period.

Now, if an individual's maximum is diminished to such a point that sixty percent of his working load is still below what the requirements of an average job on a shop floor is, then that individual has a diminished capacity for performing his job. And I don't believe that you can adequately predict exercise performance from resting lung function, except...and again, I followed the guidelines of the American Thoracis Society...except when the resting lung function is grossly disturbed. The guidelines they have offered in their recent standards for disability are fifty percent reduction...if vital capacity is fifty percent or more reduced, and FEV_1 I think was the second test, fifty percent or more reduced, it is reasonable to consider that individual disabled.

Q. To what percentage would you consider?

A. Considerably. I forget whether they consider

A. (cont'd.) him incapable of active work, but quite considerable disability. I would have to...I can refer you to that publication and I would have to just check on that point. That individual is disabled from further work. Reference Eppler in the American Review of Respiratory Diseases, within the last two years.

If that individual has a vital capacity of a bit more than that, like sixty percent of predicted, they recommend adding the diffusing capacity. If that also is fifty percent reduced, then they would assume that man is completely disabled for active work.

If, on the other hand, the values are sixty or seventy percent, then they recommend a detailed exercise study to precise what that individual's capability for adapting to exercise is, and I believe that that is an intelligent and reasonable approach to the assessment of disability.

Q. Which is a very difficult thing to do.

A. Yes, it is a very difficult thing, but it does at least use all the medical information in a logical way and minimized the dependence on prediction formulae or again in inverted commas, normal values for that individual. It minimized their dependence on whether those values are appropriate for the individual, and it maximized examining what we know about exercise adaption. And I believe it to be a rather good set of recommendations which merit consideration.

Q. Assuming one would have carried out all these steps, the patient would have, and would have come up with what be judged to be a ten percent incapacity in terms of lung function, and would have as well an x-ray of zero slash one using ILO standards, would have no clubbing, would have some crepitation and would be age about fifty-five. If you had to determine whether this man could continue to work in the mill where the standards would be, say, two fibers per c.c., never

Q. (cont'd.) exceeded except by five...say five never exceeded, two on an average of three-quarters of an hour, for instance. I know it's very hypothetical by way of a question, Dr. Becklake, but can you answer that question?

A. Are you asking what I would advise that man?

Q. Yes.

A. How has that man come to see me?

Q. As a worker.

A. Has he consulted me as a physician? So what I say to him is, I am...it's on a one-to-one basis between me as a physician whose help has been sought by a worker...I'm not now advising in any other capacity than on the clinical, one-to-one doctor/patient ratio?

Q. That's right.

A. I have seen one or two cases that fit quite closely to that category, cases in which the main complaint is breathlessness on exertion, a complaint which physicians, in my judgement, tend to underrate because they believe it's subjective, and yet it's very interesting...in all our dose-reponse relationships, breathlessness is one of the symptoms that shows a very close exposure-response relationship, so I believe that the complaint of breathlessness in the asbestos-exposed individual, even in the presence of a normal x-ray, should be taken seriously.

Then I can quote an example case of a man who was an insulator, who came with a seventy percent vital capacity and a DCO of about seventy percent expected value, exercise tests clearly showed abnormality in the adjustment to exercise such that his capacity was reduced and he could not sustain what is generally required to be the oxygen uptake for an active job on a shop floor. He had minimal x-ray changes. I believe that that decision, the information has to be given to the worker there and the decision as to whether he continues or not, in my view, under the circumstances you have described to me, comes back

A. (cont'd.) to him.

I believe that if that man was a member of my...if he was fifty-five and a member of my family, I wold recommend him not to...as I have, and I'm quoting an example...I would recommend him not to continue with further exposure because I believe that... oh, one other bit of evidence I didn't cite. This man had clear evidence on lung mechanics that he had stiff lungs, which greatly strengthened the evidence of the early x-ray changes, implying that they were more diffuse than indicated.

I might have suggested to that man a lung biopsy would be useful in your case, but I didn't believe it to be...I wouldn't necessarily have believed it to be necessary. That individual...and I told him about the situation, what I knew that exposure...withdrawal from exposure would not necessarily prevent progression of his disease, but that I considered in this hypothetical example we are talking about but which happens to match a case that I've seen recently, that I've thought that if he were my family, I would recommend him to try and find work elsewhere. I believe that he should make his decision based on information about the environment. In this instance we were talking about an insulator, and an insulator is a bad example, perhaps, because one doesn't know what the environment is, it's not as well defined.

I believe that I still, if the man had been working in the mines and the current situation was as you describe it, if the man were my family, I would recommend him, personally, to seek a job in which there was no exposure.

Now, that's what I've tried to say when I said I found support for Dr. Weill's view expressed at the Lyon Conference, that in the presence of definite parenchymal disease the prudent line would be to avoid further exposure.

Q. But as you say, it is something which a worker should also be able to as well?

A. Yes. You know, nobody necessarily takes your advice. Your kids don't, and your patients don't necessarily. So what you must do is formulate the advice in the best way that you know how, explaining the situation as well as you know how. But the decision as to what people do is their own. We know that from cigarettes, etc. People don't necessarily take your advice, but it's necessary to give them the best you know how.

Q. You would also recommend to that gentleman that you were talking about, not to smoke?

A. Yes, but perhaps he didn't already, and the one I'm thinking of didn't.

Q. You spoke as well, during the course of your examination, of data that you would be looking for to be able to determine whether standards, say now in force in the industry, are efficient. What data would that be? What period of time would you be looking at in respect of asbestosis?

A. I would like to make two points here. I made some comments about standards in this review. The purpose of writing this review was exactly as is stated here. I had worked with Dr. McDonald's team for some time, and our work had been published in epidemiological journals. I believe that it should be written in such a way that it was brought to the attention of practicing clinicians, and that its message should be written in such a way that they could use it. So this article was written to try and bridge the gap between the practice of clinical medicine and the epidemiological information.

I have not ever had it as my job to review scientific data with a view to standard setting. I included some comments here, but it would be a great mistake to think that I had ever been given that assignment, which is a very important type of assignment, the type of assignment that Professor Acheson was given, who was here yesterday, but I did mention this morning that to my knowledge the evidence...the

A. (cont'd.) base evidence that environment control was protecting health would be to study exposed populations whose exposure had been more or less in conformity with modern standards, with whatever they are. And as far as I knew, that the study proposed in Newfoundland was one of the few which was likely to address this issue. There may well be other studies of which I am not aware either underway or already published.

But I believe that you can say that the uses of epidemiology in occupational health are one, to identify problems, two, to investigate them, in particular the nature of the relationship between the health problem and the environment, three, to propose solutions, and four, to evaluate the success of those solutions.

It's that last step, I don't think has been yet done for asbestos-exposed populations.

Q. What would you be looking for in terms of reference?

A. Oh, you would be looking to see whether the prevalence of abnormality in a population exposed to levels which are thought to protect human health had in fact resulted... whether the prevalence was so low that you were entitled to conclude that human health is unlikely to suffer, and obviously you would make your measurements at different time periods over the course of exposure, and probably would be dead before the experiment had run its natural course, because clearly if you found after fifty years exposure that there was a very low prevalence of abnormality, something which the next generation of researchers will find, then you would be satisfied. But you would be making interim measurements to make certain that you weren't getting higher levels than you anticipated.

Q. Did you say that twenty-three years would be...

A. No, I said fifty years is likely to be...

Q. Fifty years?

A. Yes. Well, that's the criteria for the standards. Wasn't it a one percent risk of clinically evident asbestosis in fifty years exposure to one fiber, or something? Maybe twenty-five years to two fibers? I forget precisely how the British framed the wording which was the objective of their standard, but I think it was along those lines.

Q. Twenty-five years to two fibers?

A. Well, was it not...can anyone recall how the...I think...?

DR. UFFEN: Fifty ...

THE WITNESS: Fifty years...the one percent risk of important clinical disease with exposure to one fiber per c.c. for fifty years. I think that's...

DR. DUPRE: That, if I remember right, was the rationale for the Simpson report recommendations, which of course is not to be confused with the current standard, which remains two fibers.

THE WITNESS: Yes.

DR. DUPRE: According to my table, you've just... that a table does show, if I remember right, what...

THE WITNESS: That was their hope, yes. I think that was their...but it seems to me that it is necessary to make the interim observations to see where the work forces are responding or showing...or the health findings in work forces so exposed are consistent with these hypotheses.

So that was what I mean when I say that I think it is extremely important that the information is gathered to see that we are not kidding ourselves.

M. CASGRAIN: You have someone retired at sixty-five and starts to work twenty-five, you're just within the limit to find out, aren't you?

THE WITNESS: A. True. Unless he has been at

A. (cont'd.) a higher level, eh?

Q. Yes.

One last question, Dr. Becklake. In tab one in your literature, you mentioned that in relation to...and I'm referring to page six at this stage...

A. I'm sorry, which page?

Q. Page six of tab one. No, I'm sorry, I've got the wrong tab.

Anyway, I remember distinctly you made the statement...it's all right, Doctor, you won't need it...you stated during the course of examination-in-chief that when you did your check on the employees of the mine, you found that the dust measurements, do you not have to take them into account because of the relatively modest contribution of exposure since 1966, of dust exposure.

A. Mmm-hmm.

Q. What do you call a relatively modest contribution of exposure?

A. In this context, in this group of men, given that they already had had in the highest exposure group eight hundred million particles per cubic foot years, the likely addition might have been thirty. So thirty in eight hundred seemed a relatively small proportion. It was the proportional contribution that would have been added had we updated those dust indices.

And I repeat that the study would have been that much better if the dust indices had been updated.

M. CASGRAIN: Thanks very much, Dr. Becklake.

DR. DUPRE: Dr. Uffen, did you wish...

DR. UFFEN: Could I ask you a question at this point, because I think it follows the recent questions. It deals with disability, and I am referring particularly to your review article, tab seven, page 207, at the bottom of the...

THE WITNESS: I'm sorry. Which page?

DR. UFFEN: Page 207, at the bottom of the left column.

Is there more than one definition of disability?

THE WITNESS: Oh, yes. Many.

DR. UFFEN: Many?

THE WITNESS: Yes.

DR. UFFEN: When I read this, I find disability is, "The diminution of performance as perceived by the subject himself". There might be other definitions as perceived by the Workmens Compensation Board.

THE WITNESS: True, yes.

In fact, since this article was written or this review was written, the two terms have come to be used in a rather standard sense in terms of respiratory health assessment. Impairment to mean diminution of lung function measurements, resting lung function measurements, and also, to some extent to include an adjudgement of exercise adaptation. Disability, in at least the American jurisdiction, has come to mean something that is a legal definition.

But impairment is the word that is used by the physicians, and disability is what the lawyers do with the medical information...and believe me, it's amazing.

DR. DUPRE: Once again, to follow Dr. Uffen's point on page 207, now going into the righthand column, Dr. Becklake, just before the heading Evaluation of Changes with Time, you seem to be delivering your bottom line message to the Commission, which is that...

THE WITNESS: Yes.

DR. DUPRE: ...in arriving at the judgement on disability, it should be evaluated by appropriate exercise tests.

Now following some of your dialogue with

DR. DUPRE: (cont'd.) M. Casgrain, do I take it from this that one reason why you emphasize this is that the reserve capacity of the lung will vary considerably from individual to individual?

THE WITNESS: Yes, though more than that...yes, there is a certain amount of variation in reserve capacity of the lung. But however modest your reserve capacity, it is still really rather remarkable in terms of the capability of exchanging oxygen, providing a bigger amount of oxygen per unit time under the stress of effort, so that any lung, however poor, still usually has a very remarkable reserve capacity on effort.

The reason, my message there is directed at the importance of the exercise test, in my view, in the evaluation of a person's capability...particularly of doing exercise. I believe that physicians, particularly in this era, pronounce themselves to a degree that was utterly unjustified on the basis of resting lung function measurements, without making the appropriate exercise measurements. I believe that sentence was put in to direct them to the work of the colleagues at McMaster.

DR. MUSTARD: Can I ask a question at this point? Really what you are saying, and I put it in the simplest terms, is that if you really want to understand the capacity of a person for doing increased work, you have to be able to measure as directly as you can their capacity to take in air, particularly oxygen, and exchange it across the...

THE WITNESS: Under the stress of exercise.

DR. MUSTARD: That's right.

THE WITNESS: Mmm-hmm.

DR. MUSTARD: And that really about the only thing you have is exercise testing and measuring the actual gas exchange.

THE WITNESS: Yes.

DR. MUSTARD: That was what your term diffusion meant, etc.?

THE WITNESS: Yes, that was diffusion. Diffusion is one of the measurements, although even the diffusion capacity is less good a measurement than the measurements of the character of the adaptation to exercise frequency, VDVT ratios of the various other aspects of the exercise adaptation.

DR. MUSTARD: For my chairman's benefit, it's the oxygen in your red cells that are essential for your neurological function.

Can I say one other thing? You comment on chest x-ray so maybe, if I may sort of make a statement and you tell me whether you think it's dead center, or dead right or dead left.

Chest x-rays are a very limited way of assessing the condition of the lung.

THE WITNESS: What kind of condition do you refer to? Would you care to define it?

Chest x-rays are useful and they are part of the overall evaluation.

DR. MUSTARD: That will tell gross changes.

THE WITNESS: Yes, but they show...they show changes of increased density, and diseases like fibrosis block x-rays. Therefore, they have a particular role in the fibrotic diseases of the lung. They are less useful in the destructive diseases of the lung, so chest x-rays are useful in evaluating certain aspects of the lungs' state.

Let me add one point which I think is a very important point to make, that even in the presence of minimal or what is read as no x-ray change, lung biopsy very often shows clear abnormality, either of asbestosis or of silicosis or different other types of fibrotic disease. So that the absence of an x-ray, of a clear x-ray abnormality, does not

THE WITNESS: (cont'd.) exclude the presence of a pathological abnormality.

DR. MUSTARD: That's the point I'm trying to get at. And because of the variation in the quality of x-rays, variation in readers of x-rays, and then going back to Spitzer's article in the Canadian Medical Association Journal, it has to be a very limited test...

THE WITNESS: Yes, of minor changes. May I put early in inverted commas, changes.

DR. MUSTARD: All right. Early in the clinical sense.

THE WITNESS: Yes.

DR. DUPRE: Mr. Warren, as you can tell, the Commissioners have taken advantage of a change in pitchers to throw the ball around the infield.

Thank you for indulging us in this little exercise.

MR. WARREN: That's quite all right. The way lawyers have been kicked around here today I hardly think I should step in.

CROSS-EXAMINATION BY MR. WARREN

Q. Dr. Becklake, I don't have a lot of questions, but what I would like to do is pursue some of the puzzles, I think, that emerge from your work.

First of all, the exposure estimates which you have used in your work and Dr. McDonald has used in his work have been sufficient, as I understand it, to demonstrate a dose-response relationship between those exposures and morbidity data, and morbidity indicators.

A. Mmm-hmm.

Q. I take it that that is summarized, essentially, on page 217 of tab seven?

A. Are you referring to the table? Yes.

Q. Table eight.

A. Yes.

Q. Now, with respect to many of these parameters, let's take decreases in lung function as one indicator of morbidity, would it be fair to say that these exposure estimates were sufficient to demonstrate a pretty clear dose-response relationship?

A. We thought that that was a reasonable dose-response relationship, yes. It's a matter of interpretation, but that was how we interpreted the data, yes.

Q. I hestitate once again to talk about clearness of dose-response relationships with Dr. Uffen looking at it...

A. Right. There is no statistical analysis to support that. That's an interpretive interpretation of this table. This data was not subjected to the statistical analysis which Dr. Uffen didn't like.

Q. When Dr. McDonald was with us several weeks ago, he talked about the dose-response relationship which he was able to determine for mortality, from the same exposure estimates. I'm sure you are familiar with that work.

I think I can paraphrase him as saying that he felt they had an excellent relationship between the dose data or the exposure data as calculated by Dr. Gibbs, and the responses in terms of mortality which formed the basis for the study.

Do you think that's a fair statement?

A. Yes, I think that's a fair statement that those indices provided the basis for an exposure-response relationship which was quite clear in that data, yes.

Q. So whatever potential deficiencies there are in those exposure data, and there are always deficiencies in any exposure data, they were sufficient to establish both a prevalence in terms of morbidity outcomes and incidence in terms of mortality outcomes. Fair statement?

A. Yes.

Q. Okay. Now, again to summarize what I think you said before this morning with respect to your study indicated at tab eight, is it fair to say that your work demonstrates that, at least with respect to those data in that study, that removal is not a prevention?

A. It did not prevent the appearance of abnormality in a certain proportion of these men, no.

Q. Right. Would it also be fair to say, and I think you said something like this earlier this morning, that that is what one would expect from a biological standpoint since what we are talking about is a scar, and a scar is permanent?

A. No. I don't see that those two points follow.

Q. Okay, can you help me?

A. As far as we could tell on the evidence we had, these people developed scars who hadn't had scars there before. They must have developed them, insofar as the x-ray changes are reading scars, they must have developed them from dust that was already stored in their lungs at the time they had...up to the point of time where they no longer were exposed.

Q. That's good, because that makes the distinction that I want to make. First of all, if we have a worker who at point one, where you make your first measurement, has a scar, from a biological standpoint we should not expect that scar to go away even if the worker is removed from further exposure, correct?

A. No, I think it's unlikely. On the other hand, a scar as such, just like a scar in a bone that's healed, does not necessarily disable you, and particularly given the large reserve of the lung.

Q. There is a second category of situation in your study, and that is where on day one the worker did not have a scar, and on day two the worker did have a scar even though

Q. (cont'd.) between the time one and two, the worker was not exposed to asbestos?

A. Yes.

Q. That's true?

A. That's true.

Q. That is the essence of what that study appeared to show, that data base?

A. Yes.

Q. That is that removal does not prevent?

A. Right. Does not prevent the ravages of, presumably, what you already have there.

Q. At the same time, does that study stand for the proposition that removal does not slow down a process...

A. It doesn't give you any information on that. There is no information in the study that allows you to draw that conclusion. If we had done the study as Mr. Laskin would like us to have done it, and matched these individuals with others of the same exposure and the same type, and who continued exposure, we might have been able to answer it. But we didn't and we can't. There is no information on that.

Q. So with respect to the efficacy of removal, we do not know whether removal slows the process which is already underway?

A. We have no evidence in man on that, to my knowledge. On the other hand, instinctively we believe it to be the case.

Q. Exactly.

A. And the instinct accords with the animal data, which is quite strong and quite important, showing two things - not only does not as much abnormality and scarring develop, but it seems you are able to mobilize and get rid of a certain amount of dust. So that you may be diminishing the burden of your lungs by ceasing further exposure. That's the implication of the

A. (cont'd.) possibility from the animal work.

Q. Are the implications of the animal data with respect to this issue...let me start over again.

Do the animal data suggest that there are two separate processes at work here, or only one? In other words, is the slowing down of the possible rate of development of further fibrosis as a result of reduction of the cummulative residual dose through some clearance mechanism, or is there another mechanism in operation?

A. I don't know one, that I'm familiar with the animal work. I would have to go back to see if it was possible to interpret it in that light. I don't believe it was. I don't think I can answer that.

Q. In other words, it is possible to hypothesize that the reason there appears to be a slowing of the rate of progression is because there is a clearance of fibers which are deposited in the lungs?

A. It's possible, yes.

Q. But other hypotheses are likewise possible?

A. Yes, they are possible.

Q. But you don't have any to offer?

A. I have no evidence to offer, animal or otherwise.

Q. In your study, tab seventeen, which we have discussed here today, what you did was in 1967, conducted clinical examinations, and in some instances x-rays, of some one thousand workers, and then for those remaining alive and capable of being followed up, you attempted to repeat the process in 1974. That's right?

A. That's right.

Q. I guess, once again from a logical standpoint, it shouldn't be surprising that you saw a deterioration in the condition of those workers as a function of age over that seven

Q. (cont'd.) year period of time?

A. No. We would have expected a deterioration in lung function, and possibly in symptomotology simply as a function of age, and we were not interested in that. We were interested in the role of exposure.

Or put it this way, we expected everybody to age. We were interested to know whether there was an aging process greater than expected from natural aging, if you like.

Q. I wonder, in table one of that study, when you...table one, and really I think more importantly probably, table three...when you attempted to ascertain the relationship between age and attack progression/regression, did you adjust or correct your statistical, your proofs, to standardize them for exposure in the same sense that you standardized the exposure ones for age?

A. Yes, exactly. For each variable, for each stimulus, age, smoking and exposure, the results here are standardized for the other two. So when you are talking about smoking, we standardized for age and exposure. When you are talking about exposure, we standardized for age and smoking, and so on.

Q. So that means that the first two columns here, where we are talking about age, have been standardized in the sense that the age group that you are looking at have been adjusted in such a manner as to make the components of those age groups equivalent in terms of their exposure and in terms of their smoking history?

A. Within the limits of the statistical procedure, but that was...and they have limitations...but that was what was done, yes.

Q. I may be getting a little ahead of myself, but let me ask you this question. In response to Mr. Laskin this morning, you indicated that one possible explanation for

Q. (cont'd.) being unable to ascertain a relationship between exposure and attack progression/regression was the possibility that age had become a proxy variable for exposure.

My question is, if that were the case...or why shouldn't that question be answered by having standardized the age analysis for exposure and smoking histories?

A. You mean....I'm not sure I see how quite to answer this question. Could you repeat it? Perhaps I'll...

Q. It's logically difficult for me, too. Let me pose it again.

A dilemma which arose...let me try to take it stepwise and maybe we can both deal with it easier...a conundrum which emerges from your study is why there appeared to be no relationship, or a very weak relationship, if any, between attack progression and cummulative exposure. It's fair to say that's a central conundrum that we have to address in your study?

A. Yes, yes.

Q. A fair statement?

A. Yes.

Q. Now, one alternative explanation which I think you suggested might be that the relationship between exposure and progression was masked due to the fact that age became effectively a proxy variable for increased exposure?

A. A better indicator of the exposure if it was important in this particular group of men.

Q. Right. Now my question is, were that the case...or how can that be reconciled with the fact that this column here has been adjusted so as to equalize for exposure and smoking history and make the only variable we are looking at, age. Do you see my problem?

A. Not really.

Q. Okay, maybe you can...

A. In the slide that I showed you, or the figure here, we have four groups of...four exposure groups, each of them broken down and reconstituted to...this is figure one and two which is the page after the table you were referring to...and we have four exposure groups in which they have, the prevalences have been adjusted as they would have been if they had had common age profiles in each of them, and you see that there is no strong evidence that the prevalence increases from left to right.

If I change that round and now had six age groups, six decades by age, you would see a strong slope up to the right. In other words, if the grouping of individuals was by age, controlling for exposure within each cell, you would see a strong slope upwards with age.

So that within each exposure group there is a strong age relationship, and that's what I think this is trying to tell you.

Q. I may be just lost in a statistical conundrum here that is understandable to others and not to me, but let me see...let me tell you what's bothering me.

The profile which you were talking about...in other words, you could reconstitute or you could do a table or a figure three and four, which would display the data contained in the first two columns here of table three, with age being the variable we were looking at rather than exposure.

A. The criteria for classifying into groups, yes.

Q. Yes.

A. And giving them for a common exposure, yes.

Q. What we would see there, of course, is, as these data indicate, is a very strong relationship to age..

A. Relationship to age, yes.

Q. ...and progression?

A. Well, yes. Attack in all these features, and progression in the pleural change, you will see the ones that have

A. (cont'd.) statistical significance. It was for attack and progression with all the features, breathlessness, FEC and MMF, and progression...sorry, attack of pleural change, showing a strong relationship to age. Not all of them, but some of them.

Q. Yes. Were we to do that...I guess what's still bothering me and it may just be density, if not opacity, in another organ, but it is this. It is that as I understand this column, its attempt is to wash out the effects of exposure and to look solely at that variable, age.

A. Yes.

Q. And it shows a very strong relationship between age and progression. But shouldn't that correct...or maybe it shouldn't correct for...

A. No, because we wanted to bring out the effect of age. We have corrected for differences in dust exposure in the different age groups. We reconstituted the age groups to have comparable dust exposures, and now we have an age effect. And that's the one thing that shows a consistent increase. As age goes up, so attack rates for these features and progression rates for these features goes up.

Q. Except that I thought age in these columns was unrelated to exposure?

DR. MUSTARD: Can I ask a question?

MR. WARREN: Yeah, maybe you could. Because it's either...it's probably able to be clarified.

DR. MUSTARD: No, no. It's a subtle one that bedevils it, I think.

If you had started out in 1950, and were able to allocate one thousand workers to four different dust levels so that you had homogeneous age cohorts...

THE WITNESS: Yes, each...

DR. MUSTARD: Then if you tried to test exposure

DR. MUSTARD: (cont'd.) to dust and effects, you might have seen a positive correlation because under those circumstances you would have then had full control of the duration of exposure to the dust and the amount of exposure to the dust, and the amount, whereas in this correction that you have done here you have not been able to put a correction in for a dust total, the amount of time you had dust in your lungs.

In other words, that calculation is not done to the correction, is that correct?

THE WITNESS: Yes, that is correct, and it is conceivable one might have been...I think that is probably a reasonable way to explain it. If we had been able to set up a controlled experiment with the same age constitution in each exposure group and follow them, this age effect might not have been there. It might have then come out as a dose effect. But we weren't able to...

DR. MUSTARD: Both effects might have been there.

THE WITNESS: Both might have been there, who knows. Both might have been there.

MR. WARREN: Q. The logic is that we should always expect there would be an age effect.

THE WITNESS: A. Apart from dose. It's very difficult to separate the two, and the right way to approach it is to look at dose within given ages, within age cohorts, and we didn't have the numbers or the capability of doing that.

Q. This is a hard thing for any scientist to do, I realize, because what we are trying to do is explain a result you didn't expect.

A. Yes.

Q. Let's turn it around the other way and let's assume for a minute that the result is right...that is that there is only a weak relationship between...

A. Age and progression.

Q. Age and progression.

A. I might point out...

Q. I mean not age and progression, but exposure and progression.

A. Yes, sorry, exposure and progression.

I might point out that the same observation was made in another series of two hundred and sixty-seven films in which progression was studied in a different way, radiological progression in a different way, and the same weak relationship to dust as reflected in this index were found in that study. That's the study of Liddell and others, reported to the Royal Statistical Society, which is no doubt in Dr. McDonald's evidence.

But this was the second study in which progression in this work force was not clearly related to this exposure index, and in a sense the second surprise.

Q. Let's assume for purposes of discussion that it is a valid finding. That is, that there is a weak or no relationship between the progression and exposure...

A. Let me put it this way. I would have expected there to be a weak relationship with progression, because if the disease has already started there is some evidence to suggest that it would continue without further exposure, just on the basis of whatever the process is that has been set in train.

I was astounded that attack didn't show a closer relationship to exposure, particularly radiological attack, because the radiological changes are, of the ones we use, the more specific for asbestos exposure, and I was astounded that in view of the other data we didn't show a relationship between attacks of radiological abnormality and dose.

Q. Let's assume, because in a way it's the only evidence we have, but also let's just assume for purposes of discussion that it's a valid finding.

A. Yes.

Q. If you had to explain it or to hypothesize an explanation from a biological standpoint, what would it be?

A. Oh, it would be that in all instances the response is indeed related to the retention of dust in the lungs, that exposure is a rather poor reflection of what you retain. After all, most of what you breathe in you breathe out again, or get rid of and a very small imbalance between exposure and excretion or clearance would still result in retention, and that the fact that the amount of dust that was in the lungs of these men was largely determined by the exposure they had in the fifties and the sixties when exposure levels were so high, and that that dust...coming back to latent periods if you like...had lay in the lung and it was that dust which for some reason now was evoking the response.

Q. Okay.

A. And that's why it is linked more to age than to exposure index.

Q. Would it also be consistent with, then, the proposition that removing these people from further exposure would have little impact on their morbidity?

A. It's consistent with it, but there's absolutely no evidence for that on this data. Absolutely none at all.

Q. It doesn't prove that?

A. Oh, goodness me, no!

Q. But it's consistent with it?

A. It's consistent with the data, it's consistent with my explanation.

Q. Surely, but it's consistent with that explanation.

A. Yes, it's certainly consistent with it.

Q. Surely. And that is true even though these workers were exposed during that six year period to additional

Q. (cont'd.) dust concentrations?

A. Yes.

Q. Now, one thing that I wonder about. In terms of explaining the effect of that additional dust concentration, it is true, to be sure, that thirty particle years is a small proportion of the very highest exposure category, but if you looked at the lower exposure categories it becomes a more significant element.

A. Yes, it would become more significant in the individuals below ten, but it wouldn't greatly change the categories for all the others. I agree.

Q. In other words, the only point I'm trying to make is that with respect to the marginal effect of the interval... exposure during the interval 1967 to 1974, it's differential in its effect.

A. Yes.

Q. That is, it's got to be inconsequential for people who have eight hundred particle years, but for those who have...

A. Yes, it would be important for those with under ten, and that's assuming that those were individuals who had now moved from what was presumably a not very dusty job, because they had a low index, to a dusty job, and assuming that they had the highest average dust in the Quebec mines and mills at that time. Yes, it certainly...and it's conceivable that had we had that information we might have shown better relationships to dust. Quite conceivable. I couldn't deny it at all.

Q. Would it be fair to say that as we keep discussing these removal questions that they are not susceptible to easy answers?

A. Yes, very much so. Not susceptible to easy... to scientific and epidemiological study, and scientific and epidemiological studies haven't borne...don't provide evidence

A. (cont'd.) that allows you to draw conclusions very easily.

Q. Am I right in a little bit reading into some of the things you said today a sort of discomfort with hard and fast rules in this area, and a preference for...at least a personal preference for...the judgement of an informed clinician as to what to do?

A. I am not sure that the judgement of the informed clinician is the particularly good one to apply to the question you are asking about now as to the wisdom of withdrawal. I think that probably is more appropriately handled in the public health sense, because I don't think the individual clinician is in as good a position as the public health individual to make that judgement.

I have argued for the individual clinician's judgement in assessing disease, assessing exposure and attributability in assessing disability, and argued against hard and fast rules that say such-and-such an exposure must be there to provide such-and-such a consequence.

But the last decision about the wisdom of withdrawal I doubt is best placed in the hands of the individual physician, clinician, who, one, is not usually up on current environmental standards in the workplace of his patient, who may not have at hand all the information various governmental and other agencies have about the environment and changes in the environment, and who may not be as well read about the epidemiological information about exposure and response relationships. If I have a clinician whom I go to to seek medical advice, I want him to be shrewd in interpreting clinical information and clinical signs, but I don't necessarily expect him to be as well read in the public health sense of exposure, and that kind of decision may well be best left in the public domain where it is conceivable that you can commission reviews of

A. (cont'd.) information to address particular questions.

So I think that this is perhaps one domain in which the decision is perhaps better left in the public health domain.

Q. Yes. I think you were slightly misinterpreting me, but sort of only slightly. Let me pose it a little bit differently.

Do you think, recognizing that decisions in all these areas are inevitably public health and societal decisions, that the rules ought to be written...if there are going to be rules...with suspicion, flexibility, to permit full application of the informed judgement of a clinician?

A. In what decisions? I would like you to be more precise about the particular decisions in which you would like flexibility.

Q. Let's take the one that I think you have already addressed, and that's disability. Let's take that one.

A. The disability I feel quite strongly about. I feel rather personally insulted that it should be a legal and not a clinical decision. It seems to me it is a decision on which there is a lot of medical evidence that was brought to bear, and that perhaps simply because of the definition of how I understand the word disability...however in the States it has become accepted that clinicians have opted to use lawyers to define this word, and clinicians are supposed to work with incapacity.

Okay, that's a matter of definition. I feel more strongly about that one.

But advice on this issue, I think, as I have tried to explain to you, I think depends on other sorts of information which may not be readily...which the clinician may

A. (cont'd.) not readily have at hand.

Q. I'm not trying to get you to reverse roles in the sense that we lawyers supposedly dictate everything and doctors simply try to follow orders. I don't want to reverse roles and make it the doctors who are making all the decisions.

What I'm asking though, is the situation you envision...I'm not saying that correctly...is the appropriate solution from a public health standpoint, one which allows the full application of informed clinical judgement as far as the decision about what to be done with respect to individual workers in disability terms or other terms?

A. It's a hard one to answer and give the right balance that I want to try and give.

Just give it again?

Q. Let me...okay...

A. Can you...?

Q. I can, and I think...

A. Yes, come down. Ask the real question.

Q. Yes, well it sort of is the real question. If I understand what you are talking about, disability, let's take that one because it's easier.

Disability is, as I understand what you are saying, a question which ultimately really is a medical question.

A. I think it has an extremely strong medical component and it shouldn't be denied and it should be defined to the maximum possibility medically.

Now there is, perhaps, a legal component to it, or there is a public health component to it.

Q. Surely.

A. But it has a very strong medical component which should be recognized and defined to the full.

Q. Right. Now if we take issues such as removal, you cited, and I don't think anyone here disagrees with the

Q. (cont'd.) proposition that there are other considerations not readily apparent to the clinician, which come into play with respect to a decision like that...the most obvious of which is the exposure circumstances of the...

A. Yes, of the worker.

Q. ...of the worker's job.

A. Yes.

Q. At the same time, I take it that you would prefer a system where these kinds of public health decisions are made with full integration of the judgement of an informed clinician?

A. Yes, indeed. I would...that states it well. I think they should respect the nature of medical decisions and medical information, and clinical decisions. They should be integrated in the decision-making process. Not necessarily on the basis of every individual clinician who sees every individual patient, but the general clinical information should be integrated and expressed in some way in public health.

Q. So in other words the rules should be written in such a way as to permit the full integration of the clinician's views, not written in such a way...such a wooden way as to exclude them by arbitrary...

A. Yes, they should be brought to bear on the general decision-making process. There should be channels in which they should come in.

I don't mean that every individual clinician comes and sits on the public health board, if you see what I mean, but I think you have stated it nicely.

Q. Right. I wanted to emphasize and I didn't want to reverse roles. I didn't want the clinicians to substitute for the lawyers.

A. No, but I do think that the law needs to respect the nature of medical information, which is the thing that

A. (cont'd.) I find hardest in discussing with my colleagues in the U.S., the situations they are put into in terms of expressing medical information as yes or no, or being cornered into exact statements which deny the nature of the medical information, and I think it is...I feel for them being put in a position where they are asked to turn...to make statements which don't respect the nature of the medical information. I believe it's possible to do.

Q. Just an observation, I guess, and that is that lawyers have an adage for that same problem. It is that hard cases make bad law.

A. Yes.

Q. And it is...to try to generalize from one case to a whole class of cases often times leads to generalizations which aren't appropriate.

A. Yes.

Q. Lawyers say there is no substitute for the glacial evolution of common law, right John? I guess that's fair.

MR. WARREN: Anyway, that's all I have.

DR. DUPRE: Dr. Uffen?

DR. UFFEN: A last point for clarification. Just after Miss Jolley's letter...I'm sorry, but you seemed content or to have understood the answer, but I didn't because I didn't know what is obstructive function profile.

Can somebody explain to me what that is? Obstructive function profile?

THE WITNESS: Lung function tests can describe the...fall into different groups of abnormalities. There are some cases in which the abnormality suggests loss of volume, it's a lower than expected vital capacity, lower than expected lung volume. But the ability of the lung to blow out fast, make the forced expiratory maneuver, is well preserved.

By contrast, there are other profiles in which

THE WITNESS: (cont'd.) the lung volumes may be not restricted but may be high or above expected, and the lung can't empty quickly. Those are called obstructive profiles, and they are combinations of function abnormality that describe these two types...or should I say cases very often fall into one or another of these two types of abnormality, or they may be mixed.

DR. DUPRE: Dr. Mustard?

MR. LASKIN: Could we, just for the record, perhaps, Mr. Chairman, since Dr. Uffen and Miss Jolley referred to this letter, can we just mark it for the record as tab eighteen of exhibit twenty-nine?

EXHIBIT #29, TAB 18: The abovementioned document was then produced and marked.

DR. MUSTARD: I would like to change the tack of the questioning for a moment, and go to the type of questioning our chairman likes to ask, and that goes to what he calls the nexus aspect of asbestos disease.

Just to refer briefly to your tab six in which you try and look at the relationship between rheumatoid arthritis and asbestosis, or asbestos exposure, and then switch to tab sixteen where the...over to the transcript of the meeting that you were at and the discussion was taking place, and on page 117, the bottom, and 118, this commentary by Webster about the whole question of immunoglobulins in relation to asbestos, which of course is part of the host's defence system.

I was wondering, what developments have taken place, if any, over the last few years in our understanding about the immunological mechanisms and their expression in people exposed to asbestos, and whether any of the so-called autoimmune type of disorders aren't showing any association with asbestos exposure.

THE WITNESS: There is work from Hans Weill's group,

THE WITNESS: (cont'd.) John Salvaggio, and work from Margaret Turner-Warwick, defining the immunological, the effects on the immune system found in individuals with established asbestos disease, and there are a number of them well summarized in a paper by John Salvaggio, and they include...I'm not sure if I can give you a list, I don't think I'll try...but I could refer you to that paper.

Whether these changes are the consequence of disease or predisposed to it, I don't think is yet established. They are interpreted mostly as changes in immune competence, which is a consequence of the establishment of the disease...or thought to be associated with the development of the disease rather than predisposing towards it.

But again I think I may be facilely overinterpreting that, but that's where I would look to review that information in its most updated form.

DR. MUSTARD: Has there been any further definition of the association of conditions such as some of the forms of arthritis or lupus erythematosus, with asbestos?

THE WITNESS: To my knowledge, not. Unlike the silicosis of gold miners or hard rock miners, in which there has been an association described between lupus, scleroderma, a few of the immune diseases. To my knowledge this has not been described for asbestos, or if so it's much less..the evidence is much less strong.

DR. MUSTARD: Can I ask a question in a very direct manner? Medicine, in my view, in my belief, tends to identify those things that it is alert to, and tends not to identify some things that may be associated with the problem because people just are not registering it.

Do you know if anybody has specifically tried to take this question in the asbestos field and do a study to try to turn up whether there is a relationship?

THE WITNESS: Oh, yes, I think so. Because the people who...there are two groups again...John Salvaggio's group and Margaret Turner-Warwick's group...both tremendously interested in diseases in this area, and both in contact with asbestos-exposed individuals.

There is..also I think there is at least one prospective study trying to look at immune status and attack rate of disease in the future...looking at immune status as a risk factor to the development disease, rather than immune changes as a consequence of the disease having already developed.

DR. MUSTARD: Could you spell that one name, just so that...?

THE WITNESS: S A L V A G G I O, Salvaggio. The second one is Turner-Warwick, in the Brompton Hospital in England.

DR. MUSTARD: In, I think it's your tab seven, you admonish physicians not to use corticosteroids in treatment...

THE WITNESS: Good heavens.

DR. MUSTARD: ...of asbestosis...I think that's what you are thinking of, Mr. Chairman...

THE WITNESS: Dear me.

DR. MUSTARD: ...since corticosteroids have an effect on host resistance. Is there a basis for that?

THE WITNESS: I apologize if I...could you please show me where I said that?

DR. DUPRE: It's on page 208, righthand column, at the very bottom of the last paragraph.

THE WITNES: 208.

DR. DUPRE: "Use of corticosteroids is not advocated because the agent, asbestos, is as far as is known, fixed in the lung tissue".

THE WITNESS: Yes, I obviously wrote it and I don't even quote anybody, so I presume I must take responsibility

THE WITNESS: (cont'd.) for the statement. I would like to make the point, though, what has happened in interstitial lung disease since this was written is the work of Chrystal at NIH, on the possibility that there is an active phase of all interstitial lung diseases, a cellular phase, which is potentially reversible and in which he has advocated high doses of steroids...not in asbestos-related interstitial disease, because I don't think anybody had identified clinically the active phase of this disease...although, with one exception, alveolar proteinosis has been described, I think one or two cases, in relation to asbestos exposure, whether it's the causal relationship or not is not clear. Likewise diskomative interstitial pneumonitis has been described in an individual with asbestos exposure, and there I think steroids were used. But I think that amounts to perhaps one or two case reports.

I suppose this statement was made because at this time steroids were used in lung fibrosis, which was believed to be active, and this statement was made because at this time, I believed anyway, that there was not a reversible element.

DR. MUSTARD: Can I take you now to the question you were debating earlier this afternoon about the value of physical examination, chest x-rays and laboratory tests, and ask you if anyone has done a study of the relationship between radiation and exposure to asbestos, and lung cancer?

THE WITNESS: There is experimental work currently in France on the...on animal experimental work...on the interaction of radiation and asbestos exposure. I can't remember the name of the workers, but I believe I could look it up for you.

I'm not aware of it in man, no.

DR. MUSTARD: You are not aware of the results of their studies?

THE WITNESS: Yes, I think the effects are

THE WITNESS: (cont'd.) synergistic or whatever, they are amplifying, as I recall. I think the results were reported in the Lyon meeting. Perhaps they may be there or perhaps it may have been at the New York meeting. I suspect it's in the Lyon meeting that the results are to be found.

DR. MUSTARD: As my other question has indicated, I have a relatively restricted, narrowminded view as about the value of chest x-rays, and therefore if there is an experimental relationship in animals between exposure to asbestos and radiation, has anyone given consideration to the effect of repeated chest x-rays of workers exposed to asbestos as being...

THE WITNESS: More harmful than good? No, I'm not aware of it. As you know, chest x-rays are discontinued in tuberculosis surveillance because the yield was less than the risk was thought to be, the associated risk.

DR. MUSTARD: I think it's something that should be looked into because we find in Ontario that under the Public Hospital Act, under the testing routine, that we are required to do compulsory chest x-rays on people who have tuberculin positive. But in turn if you did the risk calculation, the risk from the radiation effects of the chest x-rays is greater than the risk from tuberculosis. I really wonder in this area, therefore, looking at the relationship and the discussion about it.

THE WITNESS: I think it's a very reasonable...

DR. MUSTARD: I think, you know, the questions have come up about the benefit of the chest x-ray there, versus the risk.

THE WITNESS: Yes.

DR. MUSTARD: So that there is the animal data suggesting that there is a synergistic effect?

THE WITNESS: However, I think the doses used were much higher than the doses likely to be encountered in the

THE WITNESS: (cont'd.) chest radiograph under clinical circumstances.

DR. MUSTARD: Now, can we turn to scarring?

THE WITNESS: Mmm-hmm.

DR. MUSTARD: The problem I have with the scarring discussion is, biologically in some circumstances as long as the injury stimulus is present, would you not get progression of lesions?

THE WITNESS: Mmm-hmm.

DR. MUSTARD: But if you can withdraw the stimulus, you can have regression of certain tissues.

THE WITNESS: Mmm-hmm.

DR. MUSTARD: I was wondering if anybody has been able to do experiments in the lung in which the stimulus is put in which induces the changes, and then...I can't think of the experiment you do, but you may know about it...then you withdraw the stimulus and study whether regression occurs.

THE WITNESS: I am not aware of any animal work, other than the exposure work, withdrawal from exposure and observing the development of responses, neoplastic as well as fibrotic responses, and the decrease in lung dust, recovered lung dust. But I'm not aware of other experiments in this field.

DR. MUSTARD: Has anyone in animal experiments used chrysotile and crocidolite in sort of the same fiber mix and given it to the animals and then withdrawn it and looked at what happened under those circumstances...to see (a) whether the chrysotile fibers...

THE WITNESS: These experiments have usually included all the fibers. The problem is they have looked at all types of fiber...I'm referring to Wagner's work...and he has looked at chrysotile, amosite and crocidolite, as far as I recall.

THE WITNESS: (cont'd.) The problem is, is what is the equivalent dose, because if you give an equivalent dose by weight, it turns out you get many more chrysotile fibers...I think it's chrysotile fibers...and to get equivalence of dose you have to get equivalence of fiber counts. Reference Davis et al at the Lyon meeting.

The appreciation of what is a comparable dose is a difficult one to work out in terms of looking at the effects of the different fibers.

That conclusion that it is equivalence of fiber numbers rather than weight I think is quoted directly either from Wagner's summary of the Lyon Conference, or from Davis's contribution to it.

DR. DUPRE: It's been a long day, Dr. Becklake. I just would like to ask you a few, very few questions.

To continue the line of questioning that Dr. Mustard discussed with you about the nexus between asbestosis and death, when I recall your testimony earlier this afternoon right side heart failure is very much associated with asbestosis?

THE WITNESS: Yes, I think you've rather overstated what I tried to say. I said that it may complicate severe asbestos-related fibrosis of the lungs, and occasionally asbestos-related pleural fibrosis in the absence of lung fibrosis.

DR. DUPRE: I guess the question that I'm going to come to is one that I've put to a number of your colleagues, including Dr. McDonald, and I keep asking it, I think, in part because I absorb this kind of technical material very, very slowly indeed when I can absorb it.

But when I look at any of a number of the mortality studies, Dr. McDonald's, any of a number of others, and then I see pneumoconiosis, or for that matter asbestosis, as the cause of death. On the basis of the education I've received so far, I have become somewhat conscious of the following, that depending

DR. DUPRE: (cont'd.) on the particular cohort and the particular situation, death certificates have sometimes been re-examined or corrected...nosologists have done some of their own work, I think in the U.S., to try to standardize interstate differences which I understand are quite considerable. But I guess when all is said and done, I'm turning to you really as a clinician.

When an individual has his death attributed to asbestosis, at least in terms of the kind of studies that have been done by the McGill faculty, will it be the case, for example, that if a particular individual who had asbestosis but whose, let us say... proximal cause of death is right side heart failure, will nonetheless be classified as having died of asbestosis, or will he show up as a heart failure death?

THE WITNESS: I can't answer for certain, but I believe in the McGill mortality studies asbestosis was taken to be a cause of death even if it wasn't cited as the primary cause of death. But you need to verify that. That would need to be verified, because when one does a mortality study one writes certain rules and one sticks with those rules, and I was not associated with that mortality study, but I think that's how the rules were written for that.

What I think people who aren't in the medical field perhaps don't appreciate is the way in which the death certificate is filled out. It is...we are taught as medical students that it is our job to give the best interpretation of the data that we know how, and basically it is to assure that death was due to natural causes.

Now, the exact hierarchy which you give the different findings, and the exact sequence of events and the way you arrange them, is dependent on a large number of things, and remember how different, the different people who fill in the death certificates. They can vary from residents to staff

THE WITNESS: (cont'd.) physicians to country doctors to specialists in different kinds of diseases.

So that it is extraordinary to me that the information derived from death certificates, which is so, if you like, imprecise in terms of pathological findings, is susceptible to analysis in giving us the useful information it does.

But...so that there are two components to the answer to your question. I believe that is how the McGill studies were done, I would like to see that verified before you accept my answer as correct, and that the way in which a death certificate is filled out is...most of us who still fill them out regard it as a statement that this man died of natural causes, rather than that we know our death certificate is going to be reviewed as part of an epidemiological study ten years down the line.

DR. DUPRE: Could I ask you this, Dr. Becklake, on the basis perhaps of your clinical experience with asbestotics who have died. In the kind of death certificate situation that one is thinking about, if an asbestotic dies of right lung failure will the certificate...sorry, right heart failure...would the certificate make primary reference to heart failure?

THE WITNESS: It's very hard to say. I can't answer that. In the Quebec scene there is, interestingly enough, Finnish analysis of causes of death in certified cases of asbestosis...Huuskonen, H U U S K O N E N, has reviewed this data which might merit looking at. It is the Finnish data that also indicates the high prevalence of indicators on the electrocardiogram of right heart stress in individuals who have asbestosis. Both those reports, which are essentially clinical reports, might be useful in your assessment of this relationship.

DR. DUPRE: One other brief line of questioning

DR. DUPRE: (cont'd.) I wanted to pursue. In your paper on tab six, and also in tab seven, you are basically examining, as I understand it, the possible relationship between rheumatoid arthritis and asbestosis, and find no apparent relation. Is that correct?

THE WITNESS: That's correct. The objective of that study was to see whether individuals who had rheumatoid complaints seemed to be at greater risk for developing radiological abnormality. The study was not designed to do that, but we had gathered questionnaire information which we believed permitted us to draw some conclusions about whether they had or had not rheumatoid complaints, and in the examination of the data did not show a relationship which...did not suggest a relationship between rheumatoid complaints and the likelihood of developing radiological disease.

It doesn't say it didn't exist, but examined in this way in this group, it didn't show. But the study, of course, was not designed to show that. It was a retrospective analysis simply to see if there was evidence in this data to suggest a relationship.

DR. DUPRE: Am I correct in understanding that there is an association that has been confirmed between rheumatoid arthritis and lung disease in coal miners?

THE WITNESS: Coal miners who had a rheumatoid tendency may develop rather unusual pulmonary manifestations which are somewhat different from what is expected. They develop a sudden crop of large, rather large lung lesions which develop over a short period of time and which tend to regress, and these are thought to be rheumatoid nodules in the lung.

But of course on an x-ray, one only sees shadows and you don't know what the nature of the lesions is. So reading a coal worker who suddenly develops these lesions is a cause for alarm.

DR. DUPRE: Now would I be correct in understanding that among coal miners the association is...exists exclusively in the following sense: That a coal miner with rheumatoid arthritis...

THE WITNESS: Or with a rheumatoid tendency. It doesn't need to have expressed itself as arthritis necessarily, and sometimes the expression of the arthritis can come later in time, after the expression of these unusual lung lesions.

DR. DUPRE: But nevertheless, the rheumatoid condition is prior to the development of...

THE WITNESS: Well, not in all cases. It's a very peculiar relationship and I would have to review it again, but the last time I reviewed, the relationship in time was unusual, the lung changes were unusual, and it is a description of what has been observed rather than an understanding of the mechanisms and sequence of events that produces them.

DR. DUPRE: When you have noticed that there is apparently no such relationship between a rheumatoid condition and asbestosis, this is based in...in what part on clinical and what part on epidemiology?

THE WITNESS: It's based on clinical information gathered on that same thousand and fifteen men, and the study was done because we had a graduate student, Dr. White, who was interested in this question, and he said, in your study in which you gathered the data for other purposes, is there any evidence that people with rheumatoid tendency, as expressed by symptoms, were at greater risk for developing x-ray abnormality. He was unable to show it. That's all the study shows.

It doesn't mean to say the relationship does not exist even in the Quebec workers, but not as we looked at it.

And as I say, you have to design a study to look at that question specifically. This was using gathered data, data gathered for another reason.

DR. DUPRE: One very last question, on page 220 of your tab seven paper, you make a closing remark which really expresses an opinion with respect to importance of properly implementing standards, whatever the standards may be, and you make the following statement, at the very end of your paper:

> "Meanwhile, there seems to be merit in deploying energy into maintaining currently-proposed standards. Three times in the twentieth century, levels have been set because the asbestos workers' health became a matter of public concern. Had the standards been more systematically adhered to, it is likely that the asbestos-related diseases would not have re-emerged as the occupational illness of the 1960's."

Could I just ask you first of all, which three standard-setting exercises in the twentieth century did you have in mind?

THE WITNESS: The first one was the British standard-setting exercises of...was it 1929/30, and the name escaped me...Merriwether, was it? Merriwether's report.

The ...I wonder what the second two were.

DR. DUPRE: But you are thinking, in any event, of standard-setting exercises that were undertaken probably in the 1930's?

THE WITNESS: That was the first one, and then what were the other two that I was thinking of?

DR. DUPRE: You wouldn't be including the 1968?

THE WITNESS: No, no. No.

DR. DUPRE: That is too recent.

THE WITNESS: The 1930's, and then...oh, yes...the American standard-setting ...

MR. LASKIN: American Industrial Hygiene.

THE WITNESS: ...was it, in about the same sort of period.

MR. LASKIN: 1935 or 1938.

THE WITNESS: Yes, something like that, and then which would be the third one that I could have referred to there?

But...

MR. LASKIN: It was re-evaluated, was it?

THE WITNESS: Was it re-evaluated?

MR. LASKIN: I think it was re-evaluated...

THE WITNESS: I haven't reviewed this data for some time, but those were the ones that I was referring to, because it was those, had they been implemented and stuck with... and stuck with in the new industries in the demolition...or in the new exposures, in the demolition industries and the shipbuilding and refitting, then it is conceivable that the outbreak, if you like, of the sixties would not have occurred.

Of course I do believe the pressures of wartime production and so on may have been somewhat responsible for that, as the physician advisory and as the surgeon general...rather the...Mr. Califano's letter to physicians implied.

But it was those standard-setting exercises that I was referring to.

Oh, the second was the second British...the other British standard-setting exercise as well, which was back in the fifties.

In any case, you've caught me out on those dates but the first was certainly the Merriwether.

DR. DUPRE: Above all I was interested in ascertaining that you were going back to time, the standard-setting exercise...

THE WITNESS: Because what we do now is relevant to twenty years from now, and that...twenty and thirty years from

THE WITNESS: (cont'd.) now, and that's really my point.

DR. DUPRE: Thank you, indeed, Dr. Becklake.
Counsel, any other questions?

MR. LASKIN: Only to thank Dr. Becklake for coming here and being most patient with us.

DR. DUPRE: Yes. Dr. Becklake, let me, on behalf of us all here, thank you.

THE WITNESS: Thank you very much.

DR. DUPRE: Thank you, indeed, ma'am.

MR. LASKIN: Ten o'clock tomorrow morning?

DR. DUPRE: We'll rise until ten o'clock tomorrow morning, is that correct?

- - - - - - - - - - - - - - - - - -

THE INQUIRY ADJOURNED

THE FOREGOING HAS BEEN PREPARED
FROM THE TAPED RECORDINGS
OF THE INQUIRY PROCEEDINGS

Edwina Macht
EDWINA MACHT